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OF THE

## FIFTH VOLUME.

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## ERRATA.

Page 7. line 15. for  $\dot{\phi} \times FG$  read  $\dot{\phi} \times FH$

ib. ib. for  $z$  read  $z$

16. 23. for § 4. read § 14

ib. ib. for  $a \cos \phi^2$  read  $a^2 \cos \phi$

205. 8. for  $\sin(n-v) =$ , &c. read  $n-v =$ , &c.

206. 23. for  $\frac{\cos v}{\cos}$  read  $\frac{\cos n}{\cos v}$

222. 12. for  $\tan \frac{A}{2} \times 45^\circ$ , read  $\tan \frac{A}{2} \times \sin 45^\circ$

281. 4. for transverse read conjugate

293. 2. for  $(1 + \epsilon)$  read  $(1 + e)$

## ERRATA IN VOL. IV.—HIST.

Page 6. line 4. for  $\tan \frac{O-0}{2}$  read  $\tan \frac{M}{2}$ . Also *ibid.* line 8. for 1 read R.

## ADVERTISEMENT.

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THE ROYAL SOCIETY OF EDINBURGH having resolved on an annual publication of their Transactions, it has become necessary to make some changes in the arrangement of the volumes. The Papers, whether Physical or Literary, are now introduced promiscuously, without being distinguished, as before, by those separate titles ; and the division into PART I, PART II, &c. is to be understood as referring only to the order of time, according to which the different collection of Papers, that are to enter into the same volume, shall happen to be published. At the end of the last Part of each volume will be given the History of the Society, including, as usual, the Biographical Accounts, the List of Members, and such details concerning the proceedings of the Society as it may be thought proper to lay before the Public.

EDINBURGH, }  
Aug. 1. 1799. }

\* \* \* The General Title will be given along with the last Part of the Volume.





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I. INVESTIGATION *of certain THEOREMS relating to the FIGURE of the EARTH.* By JOHN PLAYFAIR, F. R. S. EDIN. and Professor of Mathematics in the University of Edinburgh.

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[Read Feb. 5. 1798.]

1. **T**HE observations which have been made to determine the magnitude and figure of the earth, have not hitherto led to results completely satisfactory. They have indeed demonstrated the compression or oblateness of the terrestrial spheroid, but they have left an uncertainty as to the quantity of that compression, extending from about the one hundred and seventieth, to the three hundred and thirtieth part of the radius of the equator. Between these two quantities, the former of which is nearly double of the latter, most of the results are placed, but in such a manner that those best entitled to credit are much nearer to the least extreme than to the greatest. Sir ISAAC NEWTON, as is well known, supposing the earth to be of uniform density, assigned for the compression at the poles  $\frac{1}{230}$ , near-

PART I.

A 2 ly

ly a mean between the two limits just mentioned; and it is probable, that, if the compression is less than this, it is owing to the increase of the density toward the centre. BOSCOVICH, taking a mean from all the measures of degrees, so as to make the positive and negative errors equal, found the difference of the axes of the meridian  $= \frac{1}{248}$ . By comparing the degrees measured by Father LEISGANIG in Germany, with eight others that have been measured in different latitudes, LA LANDE finds  $\frac{1}{311}$ , and, suppressing the degree in Lapland, which appears to err in excess,  $\frac{1}{331}$  for the compression. LA PLACE makes it  $\frac{1}{321}$ ; SEJOUR  $\frac{1}{307}$ , and, lastly, CAROUGE and LA LANDE  $\frac{1}{300}$ .

THESE results, which reduce the excentricity of the meridians so much lower than was once supposed, agree well with the observations of the length of the pendulum made in different latitudes. Were the earth a homogeneous body, Sir ISAAC NEWTON demonstrated, that the diminution of gravity under the equator would be  $= \frac{1}{230}$ , expressed by the same fraction with the compression at the poles. M. CLAIRAULT made afterwards a very important addition to this theorem: for he shewed, that, if the earth be not homogeneous, but have a density that varies with any function of the distance from the centre, the two fractions, expressing the compression at the poles, and the diminution of gravity at the equator, when added together, must be of the same amount as in the homogeneous spheroid, that is, must be  $= \frac{2}{230}$  or  $\frac{1}{115}$ . Now, the second pendulum is concluded, from the best and most recent observations, to be longer at the pole than at the equator by  $\frac{1}{185}$ , and this, taken from  $\frac{1}{115}$ , leaves  $\frac{1}{304}$  for the compression at the poles.

2. But

2. BUT though  $\frac{1}{300}$ , or some fraction not very different from it, should be admitted as the most probable value of the compression, or ellipticity, as it is called, of the terrestrial spheroid, it still remains to be explained, why all the observations, considering the care with which they have been made, do not agree more nearly with this conclusion. Among the causes that may be assigned for this inconsistency, though unavoidable mistakes, and the imperfection of instruments, must come in for a part, there can be little doubt that local irregularities in the direction of gravity have had the greatest share in producing it. Of these irregularities, that which arises from the attraction of mountains has had its existence proved, and its quantity, in one case, ascertained, by the very accurate observations of the present Astronomer-Royal at Schehallien in Perthshire. We may trace the operation of this cause in many of the degrees that have been actually measured. Thus, in the degree at Turin, when divided into two parts, and each estimated separately, that which was to the north of the city, and pointed toward Monte Rosa, the second of the Alps in elevation, and the first perhaps in magnitude, was found greater in proportion than that toward the south, the plummet having been attracted by the mountain above mentioned, and the zenith made of consequence to recede toward the south. There are no doubt situations in which the measurement of a small arch might, from a similar cause, give the radius of curvature of the meridian infinite, or even negative.

BUT there is another kind of local irregularity in the direction of gravity, that may also have had a great effect in disturbing the accuracy of the measurement of degrees. The irregularity I mean is one arising from the unequal density of the materials under and not far from the surface of the earth; and this

this cause of error is formidable, not only because it may go to a great extent, but because there is not any visible mark by which its existence can always be distinguished. The difference between the primary and secondary strata is probably one of the chief circumstances on which this inequality depends. The primary strata, especially if we include among them the granite, may often have three times the specific gravity of water, whereas the secondary, such as the marly and argillaceous, frequently have not more than twice the specific gravity of that fluid. Suppose, then, that a degree is measured in a country where the strata are all secondary, and happens to terminate near the junction of these with the primitive or denser strata, the line of which junction we shall also suppose to lie nearly east and west; the superior attraction of the denser strata must draw the plummet toward them, and make the zenith retire in the opposite direction; thus diminishing the amplitude of the celestial arch, and increasing, of consequence, the geodetical measure assigned to a degree. From suppositions, no way improbable, concerning the density and extent of such masses of strata, I have found, that the errors, thus produced, may easily amount to ten or twelve seconds.

3. WHILE we continue to draw our conclusions, about the figure of the earth, from the measurement of single degrees, there appears to be no way of avoiding, or even of diminishing, the effects of these errors. But if the arches measured are large, and consist each of several degrees, though there should be the same error in determining their celestial amplitudes, the effect of that error, with respect to the magnitude and figure of the earth, will become inconsiderable, being spread out over a greater interval; and it is, therefore, by the comparison of two such arches that the most accurate result is likely to be obtained. But, in pursuing this method, since the arches measured cannot

bc

be treated as small quantities, or mere fluxions of the earth's circumference, the calculation must be made by rules quite different from those that have been hitherto employed. These new rules are deduced from the following analysis.

4. Let the ellipsis *ADBE* (fig. 1. Pl. I.) represent a meridian passing through the poles *D* and *E*, and cutting the equator in *A* and *B*. Let *C* be the centre of the earth, *AC*, the radius of the equator, = *a*, and *DC*, half the polar axis, = *b*. Let *FG* be any very small arch of the meridian, having its centre of curvature in *H*; join *HF*, *HG* cutting *AC* in *K* and *L*. Let  $\phi$  be the measure of the latitude of *F*, or the measure of the angle *AKF*, expressed, not in degrees and minutes, but in decimals of the radius 1; then the excess of the angle *ALG* above *AKF*, that is, the angle *LHK* or *GHF* will be =  $\phi$ , and therefore  $FG = \phi \times FG$ . Also, if the elliptic arch *AF* =  $z$ ,  $FG = z \times FH$ .

BUT *FH*, or the radius of curvature at *F*, is =

$$\frac{a^2 b^2}{(a^2 - a^2 \sin^2 \phi + b^2 \sin^2 \phi)^{\frac{3}{2}}} = a^2 b^2 (a^2 - a^2 \sin^2 \phi + b^2 \sin^2 \phi)^{-\frac{3}{2}}, \text{ as is}$$

demonstrated in the conic sections. Therefore, if *c* be the compression at the poles, or the excess of *a* above *b*,  $b^2 = a^2 - 2ac + c^2$ , or because *c* is small in comparison of *a*, if we reject its powers higher than the first,  $b^2 = a^2 - 2ac$ , and  $FH = a^3 (a - 2c) (a^2 - a^2 \sin^2 \phi + a^2 \sin^2 \phi - 2ac \sin^2 \phi)^{-\frac{3}{2}} = a^3 (a - 2c) (a^2 - 2ac \sin^2 \phi)^{-\frac{3}{2}}$ .

BUT  $(a^2 - 2ac \sin^2 \phi)^{-\frac{3}{2}} = a^{-3} (1 - \frac{2c}{a} \sin^2 \phi)^{-\frac{3}{2}} = a^{-3} (1 + \frac{3c}{a} \sin^2 \phi)$  nearly, rejecting, as before, the terms that involve  $c^2$ , &c. Hence  $FH = (a - 2c) (1 + \frac{3c}{a} \sin^2 \phi) = a - 2c + 3c \sin^2 \phi$ .

Now,

Now  $\dot{z} = \dot{\phi} \times FH$ , therefore  $\dot{z} = \dot{\phi} (a - 2c + 3c \sin^2 \phi)$   
 $= (a - 2c) \dot{\phi} + 3c \dot{\phi} \sin^2 \phi$ . But  $\sin^2 \phi = \frac{1 - \cos 2\phi}{2}$ , there-  
 fore  $\dot{z} = (a - 2c) \dot{\phi} + \frac{3c}{2} \dot{\phi} - \frac{3c}{2} \cos 2\phi$ , and taking the fluent  
 $z = (a - \frac{c}{2}) \phi - \frac{3c}{4} \sin 2\phi$ . To this value of  $z$  no constant  
 quantity is to be added, because it vanishes when  $z = 0$ .

THEREFORE an arch of the meridian, extending from the  
 equator to any latitude  $\phi$ , is  $= a\phi - \frac{c}{2} (\phi + \frac{3}{2} \sin 2\phi)$ .

5. THIS theorem is also easily applied to measure an arch of  
 the meridian, intercepted between any two parallels of the equa-  
 tor.

THUS, if  $MN$  be any arch of the meridian,  $\phi'$  the latitude of  
 $M$ , one of its extremities, and  $\phi''$  that of  $N$ , its other extremity,  
 we have  $AM = a\phi' - \frac{c}{2} (\phi' + \frac{3}{2} \sin 2\phi')$ , and

$$AN = a\phi'' - \frac{c}{2} (\phi'' + \frac{3}{2} \sin 2\phi''). \text{ Therefore the arch}$$

$$MN = a(\phi'' - \phi') - \frac{c}{2} ((\phi'' - \phi') + \frac{3}{2} \sin 2\phi'' - \frac{3}{2} \sin 2\phi').$$

6. IF, therefore,  $MN$  be an arch of several degrees of the me-  
 ridian, the length of which is known by actual measurement,  
 and also the latitude of its two extremities  $M$  and  $N$ , this last  
 formula gives us an equation, in which  $a$  and  $c$  are the only  
 unknown quantities. In the same manner, by the measurement  
 of another arch of the meridian, an equation will be found, in  
 which  $a$  and  $c$  are likewise the only unknown quantities. By  
 a comparison, therefore, of these two equations, the values of  
 $a$  and  $c$ , that is of the radius of the equator, and its excess above  
 half the polar axis, may be determined.

THUS, if  $l$  be the length of an arch measured,  $m$  the co-effi-  
 cient of  $a$ , and  $n$  of  $c$ , computed by the last formula; and if  $l'$   
 be

be the length of any other arch,  $m'$  the coefficient of  $a$ , and  $n'$  of  $c$ , computed in the same manner, we have  $ma - nc = l$ ,

$$\text{and } m'a - n'c = l'.$$

Whence  $a = \frac{n'l - n'l'}{mn' - m'n}$ ;  $c = \frac{m'l - ml'}{mn' - m'n}$  and  $\frac{c}{a} = \frac{m'l - ml'}{n'l - n'l'}$ . It

may be useful, in the numerical calculation, to observe also that  $c = \frac{ma - l}{n}$ .

7. THE arch of the meridian, which was measured in Peru, compared with that measured in France, will afford an example of the application of these formulas.

THE amplitude of the arch measured in Peru was  $3^\circ. 7'. 1''$ , and its length 176940 toises. To reduce this to the level of the sea, above which it was elevated 1226 toises, 66 toises must be subtracted, and again 12 toises added to adapt it to the mean temperature of the atmosphere. Thus corrected it is 176886 toises. The arch measured begun  $36''$  north of the equator, and extended to the parallel of  $3^\circ. 6'. 25''$  south; we shall suppose it to have begun under the equator, and to have extended to the parallel of  $3^\circ. 7'. 1''$ , a supposition which can produce no sensible error, and will somewhat simplify the calculation. Thus  $\phi$ , in the preceding formula, is an arch of  $3^\circ. 7'. 1''$  expressed in decimals of the radius 1, and so we have  $m = .0544009$ ,  $n = .1086408$ , and  $l = 176886$ .

AGAIN, the amplitude of the whole arch measured in France from Dunkirk to Perpignan is  $8^\circ. 20'. 2''\frac{1}{2}$ , and its length 475496 toises. The northern extremity of this arch is in latitude  $51^\circ. 2'. 1''$ , and the southern in  $42^\circ. 41'. 58''\frac{1}{2}$ . Hence  $\phi' = .8907045$ , and  $\phi = .7452459$ , and therefore  $m' = .1454586$ ,  $n' = .0585735$ ,  $l = 475496$ .

THEREFORE,  $a = \frac{n'l - n'l'}{mn' - m'n} = 3273325$  toises;

$c = \frac{m'l - ml'}{mn' - m'n} = 10917$  toises,

and  $\frac{c}{a} = \frac{1}{300}$  nearly.

PART I.

B

Wherefore

Wherefore also the longer axis of the meridian is to its conjugate, or  $a$  is to  $b$  as 300 to 299.

THIS proportion agrees well with that which was already pointed out as the most probable result, from the comparison of single degrees, and from observations of the pendulum. As these conclusions are obtained by different methods, they tend greatly to confirm one another.

8. FROM this too it seems highly probable, that the uncertainty which yet remains with respect to the true figure of the earth will be entirely removed by the measurement of some other considerable arches of the meridian. Such an arch will be furnished by the survey of Great Britain begun by General ROY, and still continued in a style of accuracy so much superior to any other system of geometrical operations that has ever yet been executed. In drawing the conclusions from observations made with such exactness, it may be necessary to employ a more accurate approximation than has been done in the preceding *formulae*, by retaining the second power of  $c$ . The equations to be resolved will thus become of the second order, but as the unknown quantities can be nearly found by the solution of a simple equation, the farther approximation to their true values will be accompanied with no difficulty.

9. CONCERNING this farther approximation it may be useful however to remark, that if  $c^2$  be retained, its coefficient in the

formula of § 4. will be  $\frac{1}{16a} \left( \phi + \frac{15}{4} \sin 4\phi \right)$ ;

and therefore in the formula of § 5. it will be

$$\frac{1}{16a} \left( \phi'' - \phi' + \frac{15}{4} (\sin 4\phi'' - \sin 4\phi') \right).$$

IF then the quantity

$$\frac{1}{16a} \left( \phi'' - \phi' + \frac{15}{4} (\sin 4\phi'' - \sin 4\phi') \right),$$

computed for any arch of the meridian, be put  $= g$ , and the same



same, computed for any other arch, be  $= g'$ , the equations of § 6. will become,

$$ma - nc + \frac{g^2}{a} = l, \text{ and}$$

$$m'a - n'c + \frac{g'^2}{a} = l'.$$

10. HERE if we put  $d$  for the value of  $a$ , as given by the formula  $\frac{n'l - n'l'}{m'n' - m'n}$ ; and  $b$  for the value of  $c$ , as given by the formula  $\frac{m'l - m'l'}{m'n' - m'n}$ , also  $v$  for the correction to be made on  $d$ , and  $u$  for the correction to be made on  $b$ , so that  $a = d + v$ , and  $c = b + u$ , by substituting these values of  $a$  and  $c$  in the two last equations we have  $mv - nu + \frac{g(b+u)^2}{d+v} = 0$ , and

$$m'v - n'u + \frac{g'(b+u)^2}{d+v} = 0.$$

Hence, rejecting all the terms that involve  $v^2$ ,  $u^2$ , or  $uv$ , we have  $dmv - dnu + gb^2 + 2gbv = 0$ ,  
and  $dm'v - dn'u + g'b^2 + 2g'bv = 0$ .

Therefore,  $v = \frac{(ng' - n'g)b^2}{(n'm - nm')d + (g'n - g'n')2b}$ , also

$$u = \frac{g'b^2(dm + 2gb) - g'b^2(dm' + 2g'b)}{dn'(dm + 2gb) - dn(dm' + 2g'b)}.$$

And, again, by rejecting those terms that are small in comparison of the rest,  $v = \frac{b^2(ng' - n'g)}{d(n'm - nm')}$ , and

$$u = \frac{b^2(g'm - gm')}{d(n'm - nm')}.$$

Thus  $v$  and  $u$  are found, and of consequence  $d + v$  and  $b + u$ , that is  $a$  and  $c$ , without neglecting any terms that are not of an order less than  $\frac{c^2}{a}$ ; and when it is considered that  $\frac{c^2}{a}$  is less than  $\frac{1}{22500}$ , it will readily be allowed that it is quite unnecessary to carry the approximation farther.

11. THE same thing that renders the comparison of large arches of the meridian useful for lessening the effect of errors arising from irregularities in the direction of gravity, makes it serve to diminish the effect of all the errors of the astronomical observations at the extremities of the arches, from whatever cause they arise. They are all diffused over a greater interval, and have an effect proportionally less in diminishing the accuracy of the last conclusion.

12. THE measurement therefore of large arches of the meridian, especially if performed in distant countries, is likely to furnish the best *data* for ascertaining the true figure of the earth; and on this account extensive and accurate surveys, such as that above mentioned, are no less interesting to science, in general, than conducive to national utility. The survey of this Island, when completed, will furnish an arch of the meridian, beginning at the same parallel where that measured in France terminates, and nearly of the same extent, so that the length of an arch of more than  $16^{\circ}$ , or almost a twentieth of the earth's circumference, will become known. The different portions of this arch compared with one another, or with the arch measured in Peru, will afford a variety of *data* for determining the true figure of the earth.

BUT surveys of the kind now referred to, afford likewise other materials from which the solution of this great geographical problem may be deduced. These are chiefly of two sorts, viz. the magnitude of arches, either of the curves perpendicular to the meridian, or of the circles parallel to the equator. Examples of the first of these have been given by General ROY and Mr DALBY; the observations which follow are directed toward both.

13. WITH respect to the measurement of arches perpendicular to the meridian, it may be observed, that the directions of gravity

gravity at different points of such arches do not intersect one another at all, unless the distances of those points from the said meridian be very small. On this account the measurement of a large arch perpendicular to the meridian would involve in it considerable difficulty; to avoid which it is necessary that the arch measured be but small, or one that does not greatly exceed a single degree. Such measurements are of course obnoxious to all the errors that arise from the deflection of the plumb-line, and cannot therefore furnish *data* for determining the figure of the earth, equally valuable with those which may be derived from large arches of the meridian. The method of determining the figure of the earth, from degrees of the perpendicular to the meridian, is not however without its advantages, and in certain circumstances is preferable to any other that proceeds by the measurement of arches equally small. This method is twofold; as a degree of the meridian may be compared with a degree of the perpendicular to it in the same latitude; or two degrees perpendicular to the meridian, in different latitudes, may be compared with one another. The advantages peculiar to each will appear from the following investigation.

14. LET it be required to find the axes of an elliptic spheroid, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the same latitude.

LET the ellipsis *ADBE* (fig. 1. Pl. I.) represent a meridian, of which a degree is measured at *F*. Let the perpendicular to the meridian in *F* meet the less axis *DE* in *R*. Then *R* will be the centre of curvature of the circle cutting the meridian at right angles in *F*; for at any point in that circle indefinitely near to *F*, the direction of the plumb-line, or of gravity, as it always passes through the axis *DE*, will cut *DE* in *R*; it will therefore  
also

also intersect FR in R, so that R is the centre, and RF the radius, of curvature of the perpendicular to the meridian. Let H be the centre of curvature of the meridian itself at F: draw FO perpendicular to DE, and let the latitude of F, or the angle OFR =  $\phi$ . Also let AC =  $a$ , CD =  $b$ , and  $a - b = c$ , as before.

THEN from the nature of the ellipsis,  $FO = \frac{a^2 \cos \phi}{\sqrt{a^2 \cos^2 \phi + b^2 \sin^2 \phi}}$ , and because  $\sin FRO : 1 :: FO : FR$ , that is,  $\cos \phi : 1 :: FO : FR$ ,  $FR = \frac{a^2}{\sqrt{a^2 \cos^2 \phi + b^2 \sin^2 \phi}}$ ; and this, therefore, is the radius of curvature of the section of the spheroid perpendicular to the meridian at F. But the radius of curvature of the meridian at F, that is  $FH = \frac{a^2 b^2}{\sqrt{a^2 \cos^2 \phi + b^2 \sin^2 \phi}}$ , therefore  $FR : FH : \frac{a^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{3}{2}}} : \frac{a^2 b^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{3}{2}}}$ , and dividing both by  $\frac{a^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{3}{2}}}$ , we have  $FR : FH :: a^2 \cos^2 \phi + b^2 \sin^2 \phi : b^2$ .

15. If then D be the length of a degree of the meridian at F, and D' the length of a degree of the circle at right angles to it,  $D' : D :: a^2 \cos^2 \phi + b^2 \sin^2 \phi : b^2$ , and  $\frac{D'}{D} = \frac{a^2 \cos^2 \phi + b^2 \sin^2 \phi}{b^2} = \frac{a^2}{b^2} \cos^2 \phi + \sin^2 \phi$ . Hence  $\frac{D'}{D} - \sin^2 \phi = \frac{a^2}{b^2} \cos^2 \phi$  and  $\frac{a}{b} = \frac{\sqrt{\frac{D'}{D} - \sin^2 \phi}}{\cos \phi}$ .

THIS last formula, therefore, gives the ratio of  $a$  to  $b$  when D, D' and  $\phi$  are known.

16. To find  $a$  and  $b$  themselves, if  $m = 57.2957$ , &c. or the number of degrees in the radius, so that  $mD' = FR$

=

$= \frac{a^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{1}{2}}}$ , and since it has been already shewn that

$$\frac{a^2}{b^2} = \frac{\frac{D'}{D} - \sin^2 \phi}{\cos^2 \phi}, \text{ or } b^2 = \frac{a^2 \cos^2 \phi}{\frac{D'}{D} - \sin^2 \phi}, \text{ therefore } mD' =$$

$$\frac{a^2}{\left( a^2 \cos^2 \phi + \frac{a^2 \cos^2 \phi}{\frac{D'}{D} - \sin^2 \phi} \times \sin^2 \phi \right)^{\frac{1}{2}}} = \frac{a}{\cos \phi \left( 1 + \frac{\sin^2 \phi}{\frac{D'}{D} - \sin^2 \phi} \right)^{\frac{1}{2}}} \text{ and } a =$$

$$mD' \cos \phi \sqrt{1 + \frac{\sin^2 \phi}{\frac{D'}{D} - \sin^2 \phi}}.$$

$$\text{Now, } 1 + \frac{\sin^2 \phi}{\frac{D'}{D} - \sin^2 \phi} = \frac{\frac{D'}{D}}{\frac{D'}{D} - \sin^2 \phi} = \frac{1}{1 - \frac{D}{D'} \sin^2 \phi}, \text{ therefore}$$

$$a = \frac{mD' \cos \phi}{\sqrt{1 - \frac{D}{D'} \sin^2 \phi}}.$$

17. THIS value of  $a$  is very convenient for logarithmical calculation; for if  $\sin \phi \sqrt{\frac{D}{D'}}$  be computed, it will always be less than 1, because  $D'$  is greater than  $D$ , and therefore may be taken for the sine of an arch  $\psi$ , of which arch  $\sqrt{1 - \frac{D}{D'} \sin^2 \phi}$  will of course be the cosine, so that  $a = \frac{mD' \cos \phi}{\cos \psi}$ .

THE same method may be used for finding  $\frac{a}{b}$  from the formula in § 15.

IN the same manner that  $a$  has been found, we will obtain

$$b = \frac{mD' \cos \phi}{\left( 1 - \frac{D}{D'} \sin^2 \phi \right) \sqrt{\frac{D'}{D}}}.$$

IF we examine these formulas in the extreme cases, viz. when  $\phi = 90^\circ$ , and when  $\phi = 0$ , we shall have in the former case  $a = 0$ , because  $\cos \phi = 0$ , and also  $D' = D$ , so that

1 —  $\frac{D}{D'} \sin^2 \phi = 0$ . Here therefore  $a$  is indefinite, and may be of any magnitude whatever; and it is evident that this is the result which the formula ought to give: because at the pole, or when  $\phi = 90^\circ$ , the perpendicular to the meridian is itself a meridian, and therefore the measurement of the two degrees,  $D$  and  $D'$ , is but the same with the measurement of one degree.

WHEN  $\phi = 0$ , that is at the equator, the circle perpendicular to the meridian is the equator itself, and we have then  $a = mD'$ ,  $a$  being determined in this case by the degree of the equator alone. Here also we have  $\frac{a}{b} = \sqrt{\frac{D'}{D}}$ , which is known to be true.

18. THE preceding formulas may be rendered more simple, if we aim only at an approximation, which indeed is all that is necessary in this inquiry. Since  $c$  denotes the compression, or since  $a - c = b$ , and therefore  $a^2 - 2ac = b^2$  nearly, consequently the radius of curvature of the meridian at  $F$ , that is

$$mD = \frac{a^2 (a^2 - 2ac)}{(a^2 - 2ac \sin^2 \phi)^{\frac{3}{2}}} = \frac{a^3 (a - 2c)}{a^3 (1 - \frac{2c}{a} \sin^2 \phi)^{\frac{3}{2}}} =$$

$(a - 2c) (1 - \frac{3c}{a} \sin^2 \phi)$ , or  $mD = a - 2c + 3c \sin^2 \phi$ . In the same manner  $mD' = a + c \sin^2 \phi$ . From these equations we obtain, rejecting always the higher powers of  $c$ ,

$$c = \frac{m(D' - D)}{2 \cos \phi^2}, \quad a = mD' - \frac{m(D' - D) \sin^2 \phi}{2 \cos \phi^2}; \quad \text{and} \quad \frac{c}{a} = \frac{D' - D}{2D' \cos \phi^2}.$$

THESE formulas may be transformed into others a little more convenient for computation, by putting  $\sec \phi^2$  instead of  $\frac{1}{\cos \phi^2}$ , and  $\tan \phi^2$  instead of  $\frac{\sin \phi^2}{\cos \phi^2}$ ; we have then,

$$c = \frac{m}{2} (D' - D) \sec \phi^2,$$

$$a = mD' - \frac{m}{2} (D' - D) \tan \phi^2, \quad \text{and}$$

$$\frac{c}{a} = \frac{(D' - D)}{2D'} \sec \phi^2.$$

19. WE may apply these formulas to the computation of  $\frac{c}{a}$ , &c. from the degrees of the meridian and perpendicular, measured in the south of England. We find, in one example, (*Phil. Transf.* 1795, p. 537.), that  $D = 60851$  fathoms,  $D' = 61182$ , the latitude, or  $\phi$  being  $= 50^{\circ}. 41'$ . From this  $\frac{c}{a} = \frac{D' - D}{2D' \cos^2 \phi} = \frac{331}{2 \times 61182 \times (\cos 50^{\circ}. 41')^2} = \frac{1}{148.4}$ , which is nearly the same result with that deduced in the passage just referred to. Indeed the solution of this problem, contained in the *Trigonometrical Survey*, is quite unexceptionable; and the theorems here offered are not given as containing a more accurate solution, but one that is in some respects more simple.

THE above compression, if the remarks already made be well founded, is much too great, being more than double of what was obtained from comparing the whole arch of the meridian measured in France with the whole of that measured in Peru. At the same time it is right to observe, that all the other comparisons of the degrees of the meridian, with those of the curve perpendicular to it, made from the observations in the south of England, agree nearly in giving the same oblateness to the terrestrial spheroid. For this circumstance, it is certainly not easy to account; the unparalleled accuracy with which the whole of the measurement has been conducted, makes it in the highest degree improbable that it arises from any error; and even if errors were to be admitted, it is not likely that they should all fall on the same side. The authors of the *Trigonometrical Survey* seem willing, therefore, to give up the elliptic figure of the earth, (*Ibid.* p. 527.); but before we abandon that very natural and simple hypothesis, it may perhaps be worth while to attend to the following considerations.

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20. IN the part of England, where the measures we are now treating of have been taken, the strata are of chalk, and though of great extent, are bordered, on all the sides that we have access to examine by strata much denser and more compact. Toward the west the chalk is succeeded by limestone, and that limestone by the primitive schistus and granite of the west of Devonshire and of Cornwall. On the east we may suppose that something of the same kind takes place, though the sea prevents us from observing it, as the chalky and argillaceous beds extend in this direction to the coast, and probably to some distance beyond it. Now the meridian of Greenwich may be considered as dividing the tract of country, occupied by these lighter strata, into two parts, in such a manner, that the plummet being carried to a distance from it, either east or west, approaches to the denser strata, and is of course attracted by them, so that the zenith is forced back, as it were, to the meridian of Greenwich, and does not recede from it, in the heavens, at so great a rate as the plummet itself does, on the earth. Hence the longitudes from this meridian, estimated by the arches in the heavens, intercepted between the zenith and the said meridian, will appear less than they ought to do; and too much space on the surface of the earth will of consequence be assigned as the measure of a degree. In this way  $D'$  is made too great; and we may suppose the circumstances such that  $D$ , on going north or south, is not enlarged in the same proportion; hence  $\frac{D' - D}{D'}$  will be augmented, and of course  $\frac{c}{a}$  will be represented as too great. This explanation may perhaps appear very hypothetical, and it is certainly proposed merely as a hypothesis. It is a hypothesis, too, that lays claim only to a temporary indulgence, as it is proposed at the very moment when it may be brought to the trial, and when, by a further continuation of the survey toward the north,



north, it will probably be determined how far the distribution of the strata of this country affects the direction of gravity. It will indeed be curious to remark what irregularities take place on advancing into the denser strata of the north. The limestone and sandstone strata of the middle part of the Island will succeed to the chalk of the south, the primitive and denser strata still occupying the west, at least at intervals, as in Wales, Cumberland, and Galloway. Further to the north, that is, beyond the Tay, the strata become entirely primitive, most of them of the densest kind, and in the interior of the Island, with a very few exceptions, continue the same to its most northern extremity. In the survey of Britain, therefore, several situations must occur where the plummet, passing from lighter to denser strata, ought to give indications of some irregularities in the direction of the gravitating force. It will be seen hereafter how far these conjectures are verified by experience.

21. A REMARK, that is in no danger of being reckoned hypothetical, is, that the conclusion derived from the comparison of degrees of the meridian, with degrees of the circle perpendicular to it, becomes of necessity more liable to error as we advance into higher latitudes. The reason is, that whatever error is committed in determining the magnitude of  $D' - D$ , must be multiplied into the square of the secant of the latitude, in order to give its full effect in changing the value of the fraction  $\frac{c}{a}$ . For it has been shewn, that  $\frac{c}{a} = \frac{1}{2} \left( \frac{D' - D}{D'} \right) \sec^2 \phi$ ; now, if we suppose the error committed in ascertaining  $D' - D$  to be in all cases the same, the error of the fraction  $\frac{D' - D}{D'}$  will also be in all cases nearly the same, the denominator  $D'$  being but little affected either by the supposed error, or by the change of latitude. But this error, which may thus be considered as a constant

quantity, when multiplied into  $\frac{1}{2} \sec^2 \phi$ , gives the variation or error in  $\frac{c}{a}$ , which error therefore increases, *cæteris paribus*, as the square of the secant of the latitude, so that, on approaching the pole, it increases without limit, and is ultimately infinite. Comparisons of this kind may therefore be expected to give results the more accurate the nearer they are to the equator, under which circle they will be the most accurate of all. Here, again, however, another circumstance must be taken into consideration, viz. that the method of ascertaining the differences of longitude by the convergency of the meridians, so convenient in surveys of this kind, is applicable only in high latitudes. In a trigonometrical survey, therefore, of a country lying much farther south than Britain, a different method of ascertaining the longitudes of places must necessarily be adopted.

22. THE theorems, which were next proposed to be considered, are those that determine the figure of the earth from the measures of degrees of the curve perpendicular to the meridian, in different latitudes. For this purpose let  $D'$  be a degree of one of these curves, in the latitude  $\phi'$ , and  $D''$  a degree of one them, in another latitude  $\phi''$ . Then  $c$  being the compression, as before, we have by § 18.  $mD' = a + c \sin^2 \phi'$ ,

and also  $mD'' = a + c \sin^2 \phi''$ .

Hence  $m(D' - D'') = c (\sin^2 \phi' - \sin^2 \phi'')$ , and

therefore  $c = \frac{m(D' - D'')}{\sin^2 \phi' - \sin^2 \phi''}$ .

THIS formula may be rendered more convenient for calculation, by considering that  $\sin^2 \phi' = \frac{1 - \cos 2\phi'}{2}$ , so that

$$\sin^2 \phi' - \sin^2 \phi'' = \frac{1 - \cos 2\phi' - 1 + \cos 2\phi''}{2} = \frac{\cos 2\phi'' - \cos 2\phi'}{2}. \quad \text{But}$$

$\cos 2\phi'' - \cos 2\phi' = 2 \sin (\phi' + \phi'') \times \sin (\phi' - \phi'')$ , wherefore

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$\sin^2 \phi' - \sin^2 \phi'' = \sin(\phi' + \phi'') \times \sin(\phi' - \phi'')$ , and

$$c = \frac{m(D' - D'')}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}.$$

23. IN the same manner, because  $mD' = a + c \sin^2 \phi'$ , by substituting for  $c$ , we have

$$mD' = a + \frac{m(D' - D'') \sin^2 \phi}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}, \text{ and}$$

$$a = mD' - \frac{m(D' - D'') \sin^2 \phi}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}.$$

24. LASTLY, since  $mD' = a + c \sin^2 \phi'$ ,

$$\text{and } mD'' = a + c \sin^2 \phi'',$$

dividing the first of these equations by the second, and rejecting the higher powers of  $c$ , we have

$$\frac{D'}{D''} = 1 + \frac{c}{a} (\sin^2 \phi' - \sin^2 \phi''), \text{ and therefore,}$$

$$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\sin^2 \phi' - \sin^2 \phi''}. \quad \text{Hence also}$$

$$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}; \text{ or more conveniently for calculation by logarithms, } \frac{c}{a} = \frac{D' - D''}{D'' \sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}.$$

25. WE may compare this value of  $\frac{c}{a}$  with that obtained in § 18. from other data, in order to determine which of the two methods of finding  $\frac{c}{a}$  is to be preferred, under given circumstances. Suppose, for instance, a degree of the curve perpendicular to the meridian, in the latitude  $\phi'$  to be  $D'$ , and a degree of the meridian itself in the same latitude to be  $\Delta'$ ; it is required to find in what other latitude  $\phi''$ , a degree  $D''$ , perpendicular to the meridian, must be measured, in order that the comparison of  $D'$  and  $D''$ , and of  $D'$  and  $\Delta$ , may give values of  $\frac{c}{a}$ , in which the probable error is the same.

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HERE, agreeably to an observation already made, we may, in order to estimate the error produced in  $\frac{c}{a}$ , in consequence of an error in the determination of  $D'$ , and  $D''$ , and  $\Delta$ , suppose the error to affect  $D' - D''$ , or  $D' - D''$  only, without paying any regard to the variation of  $D'$  in the denominator. Therefore, since by § 18. we have  $\frac{c}{a} = \frac{D' - \Delta}{2D' \cos^2 \varphi'}$ , and again, by § 24.  $\frac{c}{a} = \frac{D' - D''}{D'(\sin^2 \varphi' - \sin^2 \varphi'')}$ , if we suppose equal errors in determining  $D' - \Delta$ , and  $D' - D''$ , and also that these are the only errors, their effect will be the same, in both cases, if  $2 \cos^2 \varphi' = \sin^2 \varphi' - \sin^2 \varphi''$ . Now, if we suppose  $\varphi''$  the quantity sought, and add  $\cos^2 \varphi'$  to both sides of the preceding equation, then  $3 \cos^2 \varphi' = \sin^2 \varphi' + \cos^2 \varphi' - \sin^2 \varphi'' = 1 - \sin^2 \varphi'' = \cos^2 \varphi''$ . The latitude  $\varphi''$  therefore must be such, that  $\cos \varphi'' = \sqrt{3} \times \cos \varphi'$ . If, therefore,  $\varphi'$  be such that  $\cos \varphi' = \frac{1}{\sqrt{3}}$ , the cosine of  $\varphi''$  will be  $= 1$ , and  $\varphi''$  therefore  $= 0$ . Now,  $54^\circ. 44'$  is the arch of which the cosine  $= \frac{1}{\sqrt{3}}$  nearly, therefore, if a degree of the meridian, and of the perpendicular to it, be measured in latitude  $54^\circ. 44'$ , the comparison of these with one another will give a result as accurate as if the degree of the perpendicular, in that latitude, were compared with the degree at the equator, and more accurate of consequence than if any other degree of the perpendicular to the meridian, were to be compared with  $D'$ .

26. HENCE, also, the comparison of the degree of the meridian, and of the perpendicular to it, in the south of England, is better than if a degree of the perpendicular measured in that latitude were compared with a degree at the equator. For if, in the equation  $\cos \varphi'' = (\cos \varphi') \times \sqrt{3}$ , we make  $\varphi' = 50^\circ. 41'$ , (or any thing less than  $54^\circ. 44'$ ),  $\varphi''$  will come out impossible.

27. It may be shewn, too, nearly in the same manner, that if a degree of the perpendicular to the meridian were measured in Siberia, as far north as the latitude of  $70^\circ$ , supposing that to be possible, and compared with a degree in latitude  $45^\circ$ , or even considerably farther south, it would not give a result so exact as the degree of the meridian and perpendicular measured in the south of England. This shews, that the method of ascertaining the figure of the earth, proposed by the authors of the *Trigonometrical Survey*, (*Phil. Trans.* *ibid.* p. 529.), as a subject of future inquiry, is less exact than that which is founded on their own observations.

28. WE may also ascertain, by the same means, the relative accuracy of the method of finding the figure of the earth, from the comparison of a degree of the meridian with a degree of the perpendicular in the same latitude, and of the method of resolving the same problem by the comparison of two degrees of the meridian in different latitudes.

IF, then,  $D$  be a degree of the meridian, and  $D'$  of the perpendicular, in latitude  $\phi$ , and if  $\Delta$  be a degree of the meridian in a different latitude  $\phi'$ , it is required to find whether the most accurate value of  $\frac{c}{a}$  will be found, by comparing  $D$  and  $D'$ , or  $D$  and  $\Delta$ .

SINCE we have, by what has been already stated, § 4.

$$mD = a - 2c + 3c \sin^2 \phi, \text{ and}$$

$$m\Delta = a - 2c + 3c \sin^2 \phi', \text{ we have also}$$

$$\frac{D}{\Delta} = 1 + \frac{3c}{a} (\sin^2 \phi' - \sin^2 \phi) \text{ and therefore,}$$

$$\frac{c}{a} = \frac{D - \Delta}{3\Delta (\sin^2 \phi - \sin^2 \phi')}.$$

Now, it has been already shewn, that, by comparing  $D$  and  $D'$  we have  $\frac{c}{a} = \frac{D' - D}{2D' \cot^2 \phi}$ . Supposing, therefore, equal errors

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to be committed in the determination of  $D - \Delta$ , and of  $D' - D$ , and also paying no regard to the inequality of  $\Delta$  and  $D'$  in the denominators of these fractions, as it is not so great as materially to affect the quantity that is sought for here, we shall have the errors in  $\frac{c}{a}$  nearly the same in both formulas, when  $\phi$  and  $\phi'$  are such that  $2 \cos^2 \phi = 3 \sin^2 \phi - 3 \sin^2 \phi'$ , or when  $\frac{2}{3} \cos^2 \phi = \sin^2 \phi - \sin^2 \phi'$ , that is, adding  $\cos^2 \phi$  to both sides,  $\frac{5}{3} \cos^2 \phi = \sin^2 \phi + \cos^2 \phi - \sin^2 \phi'$ , and, therefore,  $\frac{5}{3} \cos^2 \phi = 1 - \sin^2 \phi' = \cos^2 \phi'$ , or  $\cos \phi' = (\cos \phi) \sqrt{\frac{5}{3}}$ .

29. If, therefore,  $\cos \phi = \sqrt{\frac{3}{5}}$ ,  $\cos \phi' = 1$ , that is  $\phi' = 0$ , so that  $\Delta$ , the second of the degrees of the meridian, must in this case be under the equator. But  $\sqrt{\frac{3}{5}}$  is the cosine of  $39^\circ. 14'$ , in which latitude therefore if  $D$  and  $D'$  be measured, the result, by comparing them with one another, is as exact as if  $D$  were compared with the degree under the equator. Hence, if  $D$  and  $D'$  are measured in a lower latitude than the above, the result will be more exact, than if  $D$  were compared with the degree at the equator.

If we suppose  $D$  and  $D'$ , measured in the south of England, so that  $\phi = 50^\circ. 41'$ ; then we will have  $\phi' = 35^\circ. 7'$ , so that  $D$  must be compared with a degree of the meridian as far south as  $35^\circ. 7'$ , in order that the result may be as good as when  $D$  and  $D'$  are compared with one another.

From this it is evident, that the method of comparing degrees of the meridian, and perpendicular in the same latitude, has even an advantage over the comparison of degrees of the meridian in different latitudes, unless these last are taken at a considerable distance from one another.

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IN this way may many useful conclusions be derived concerning the degree of credit due to measurements already made, as well as with respect to the selection of the places where they are to be made hereafter. On these I shall enter no further at present, and shall only add, that, besides the advantages or disadvantages which the method of comparing together degrees of the meridian and perpendicular in the same latitude has, and which are subjects of calculation, it has another advantage, which in the case of the British survey is undoubtedly very great, viz. that all the *data* are furnished from one system of trigonometrical operations; executed according to the same plan, with the same instruments, and by the same observers.

30. ONE other application of geometrical measurements to discover the figure of the earth yet remains to be considered. This is the comparison of an arch of the meridian with an arch of a parallel of latitude which crosses it. The measure of a parallel of latitude can be executed readily, and is not confined to a small arch as in the case of a perpendicular to the meridian. The plumb-line, while it is carried along the circumference of a parallel to the equator, tends continually to the same point in the earth's axis, so that there is no difficulty in ascertaining the amplitude of the arch measured, providing there be no unusual disturbance of the direction of gravity. As an arch of a parallel to the equator, however, is not the shortest line between two points on the surface of the spheroid, the measurement along that surface will not give the length of the arch truly. To obviate this difficulty, it is only necessary to follow the method so properly introduced into the *Trigonometrical Survey*, of reducing the measures, both of lines and angles, to the chords and to the planes of the rectilinear triangles contained by them. In this way, the chord of an arch of a parallel of latitude may be determined, however great the arch; and it is worthy of being remarked, that, whatever be the deflections of the plumb-line at the intermediate sta-

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tions, when the reductions are all properly made, the length of the chord measured will not be affected by them; the amplitude of the arch indeed may be affected by such deflections, if they happen at its extremities; but the effect of this error will be rendered the less, the greater the arch that is measured. We may suppose, therefore, that the chord of a large arch of a parallel of latitude is measured, and the amplitude of the arch itself at the same time accurately ascertained. This last may be done, either by measuring the convergency of the meridians, if it be in a high latitude, or by any other method of ascertaining differences of longitude which admits of great accuracy. The chord being thus given in fathoms, and the arch subtended by it being given in degrees and minutes, the radius of the parallel itself becomes known.

31. Now, if we would compare the radius of a parallel thus found, with a large arch of the meridian, we shall have by that means a determination of the figure of the earth, not less to be relied on than that given in the beginning of this paper. The investigation is easy by help of the theorems in § 5. and 6. Let FO be the radius of a parallel to the equator, which passes through F, the latitude of which is  $\phi$ , and is supposed known; and let FO found by the method just described be  $= r$ , then,

as in § 4.  $r = \frac{a^2 \cos \phi + b^2 \sin \phi^2}{\sqrt{a^2 \cos \phi^2 + b^2 \sin \phi^2}} = \frac{a \cos \phi^2}{a \sqrt{1 - \frac{2c}{a} \sin \phi^2}}$ , according to

the method of reduction followed in the preceding articles of

this paper. Then, because  $\sqrt{1 - \frac{2c}{a} \sin \phi^2} \approx 1 + \frac{c}{a} \sin \phi^2$  near-

ly, we have  $r = a \cos \phi (1 + \frac{c}{a} \sin \phi^2) = a \cos \phi + c \sin \phi^2 \cos \phi$ ,

or if we divide by  $\cos \phi$ ,  $\frac{r}{\cos \phi} = a + c \sin \phi^2$ . Let  $\frac{r}{\cos \phi} = l$ , then

$l = a + c \sin \phi^2$ .

32. AGAIN,



32. AGAIN, if  $\phi'$  and  $\phi''$  are the latitudes of the extremities of an arch of the meridian, the length of which has been measured, and found  $= l'$ , then, according to § 5. we have

$$l' = a (\phi'' - \phi') - \frac{c}{2} ((\phi'' - \phi') + \frac{3}{2} (\sin 2\phi'' - \sin 2\phi'))).$$

If, therefore,  $m$  be the coefficient of  $a$ , in the former equation, and  $n$  the coefficient of  $c$ ; and if  $m'$  be the coefficient of  $a$ , in the latter equation, and  $n'$  of  $c$ , we have, as in § 6.

$$a = \frac{n'l - nl'}{mn' - m'n}, \text{ and } c = \frac{m'l - ml'}{mn' - m'n}, \text{ or since } m = 1,$$

$$a = \frac{n'l - nl'}{n - m'n}, \text{ and } c = \frac{m'l - l'}{n - m'n}; \text{ also } \frac{c}{a} = \frac{m'l - l'}{n'l - nl'}.$$

33. IN this way of determining  $a$  and  $c$ , the parallel of latitude may either intersect the arch of the meridian measured or not. If it intersect that arch, this method may have the same advantage that was taken notice of in another solution, viz. that the whole of the *data* may be furnished from the same system of trigonometrical operations. Thus, in the survey of Great Britain, an arch of 5 or 6 degrees of a parallel to the equator might be measured, and compared with the whole length of the meridian, comprehended between the northern and southern extremities of the Island, amounting nearly to 9 degrees.

IT is plain, from what has already been said, that the result deduced from this comparison would possess every advantage, and would be entitled to more credit, than any determination of the figure of the earth that is yet known.

34. ON the supposition that, in a survey of a country, the measurement is made along a series of triangular planes, all given in position and magnitude, there is yet another method of determining the figure of the earth, more general than any of the former. On the supposition just mentioned, it is evident, that the length of a straight line, or chord, drawn from a given

angle of any one of these triangles, to a given angle of any other of them, may be found by trigonometrical calculation. Let the latitudes be observed at the extremities of this chord, and also the difference of longitude; then, from the nature of an ellipsoid, the length of this same chord may be expressed, in terms of the axes  $a$  and  $b$ , together with the latitudes of the extremities of the chord, and the difference of longitude between them; and this expression being put equal to the length of the chord measured will give an equation, in which all the quantities are known, except  $a$  and  $b$ . Further, if  $a = b + c$ , and if the said expression be reduced into a series, with the powers of  $c$  ascending, that series will converge very rapidly, because  $c$  is small in respect of  $a$ ; then, for a first approximation, we may reject all the terms that involve the powers of  $c$  higher than the first, by which means we shall have a simple equation of the form  $ma + nc = l$ , where  $m$  and  $n$  are functions of the latitudes and difference of longitude, and  $l$  is the length of the chord.

Now, if a similar equation be derived from the measurement of any other chord, these two equations will give  $a$  and  $c$  in the same manner as in § 6.; and thus, from the measurement of any two chords, the figure of the earth will be determined.

35. THE length of the chords, thus measured, should be great, so that they may, if possible, subtend angles of several degrees, and their position will be most favourable when one of them is in the plane of the meridian, and the other nearly at right angles to it. The numerical computation will be found less laborious than might be imagined; but the complete solution of the problem, and the full detail of the investigation, I am under the necessity of delaying to some future communication.

THERE seems to be but one difficulty of any consequence that stands in the way of this method of determining the figure of the earth. It arises from this, that the ascertaining the position

tion of the supposed series of triangular planes relatively to one another, involves in it the allowance to be made for the terrestrial refraction, which it must be confessed is not accurately known, and is the more difficult to determine, that it is unavoidably combined with the irregularities in the direction of gravity. It is possible, indeed, to separate these two sources of error, but not without a system of experiments instituted directly for that purpose.

36. THE determination of the difference of longitude, which enters necessarily into this problem, except in the case when both chords are in the direction of the meridian, must also be performed with great accuracy. Among the different ways of doing this, that which proceeds by observing the convergency of the meridians, though the best accommodated to the nature of a trigonometrical survey, is not the least liable to objection. For, not to mention that it is only practicable in high latitudes, we must observe, that it always implies a correction on account of the ellipticity of the meridian, which is therefore necessarily hypothetical, and depends on the very thing that is to be found. This inconvenience, however, may be obviated by repeated approximations, and by an accurate solution of *spheroidal* triangles. On this latter subject it was my intention to offer to the Society some theorems, that contain more direct and fuller rules for this kind of trigonometry than any that I have yet met with. I am under the necessity, however, of reserving these, as well as the solution of the problem above mentioned, for the subjects of some future communication. In the mean time, I think it is material to observe, that the principle laid down by Mr DALBY, viz. that in a spheroidal triangle, of which the angle at the pole and the two sides are given, the sum of the angles at the base is the same as in a spherical triangle, having the same sides, and the same vertical angle, is not strictly true, unless the excentricity of the

the spheroid be infinitely small, or the triangle be very nearly isosceles. The application of the principle may therefore lead into error, unless it be made with due attention to these restrictions. The gentleman, just named, will forgive a remark, which I certainly should not have made, if I had been less interested for the success of the work, in which he has assisted with so much ability.

CHAP. II.

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II. ACCOUNT of certain PHENOMENA *observed in the AIR VAULT*  
*of the FURNACES of the DEVON IRON WORKS\**; together  
with some practical REMARKS on the MANAGEMENT of  
BLAST FURNACES. By Mr ROEBUCK, in a LETTER to Sir  
JAMES HALL, Bart. Communicated by Sir JAMES HALL.

[Read, July 2. 1798.]

SIR,

I HAVE examined my memorandums, concerning the observations I made on the condensed air in the *air vault* of the Devon Iron Works, near Alloa; and, according to your request, I now transmit you an account of them; and also of an experiment I made, when a partner and manager of these works, in order to increase the produce of blast furnaces.

THE two blast furnaces at Devon are of large dimensions, each being 44 feet high, and about 13 feet wide in the boshes, or widest part, and are formed on a steep bank, by two pits sunk in a very solid stratum of coarse grained freestone.

THESE pits were afterwards shaped and lined in the usual manner of blast furnaces, with common bricks and fire bricks, and the hearth was laid with large blocks of the stone that had been dug out, and which serve the purpose of fire stones. At the back of the two furnaces, next the bank, the air vault is excavated

\* THESE iron works are on the banks of the river Devon, which runs into the Frith of Forth near Alloa. They are 3 miles from Alloa, and 8 from Stirling.

cavated, and formed by a mine drove in the solid rock, distant from the furnaces about 16 feet. The bottom of the air vault is only about 4 feet higher than the level of the bottom of the furnaces. This vault has an aperture at one end to receive the air from the blowing machine, and has two at the opposite end, one of which receives the eduction pipe, and the other is a door to give admittance occasionally into the vault. As the rock is extremely close and solid, the vault is dry, except that a little water ouzes very gently from the side next the bank in small drops, and does not appear to exceed an English pint in 24 hours.

THESE furnaces are provided with air, or blast, as it is termed, by the means of a fire-engine of the old, or NEWCOMEN'S construction. The diameter of the steam cylinder is  $48\frac{3}{4}$  inches; and the square area of its piston being about  $1866\frac{1}{2}$  square inches, the power of this sort of engine cannot be rated at more than 7 lb. to the square inch, amounting in all to about 13062 lb. This power was employed to work an air pump, or blowing cylinder, of 78 inches diameter, and about 7 feet long. The number of square inches on the piston of the air pump is 4778, and therefore this area, being multiplied by  $2\frac{3}{4}$ , will produce 13139, being a resistance that nearly balances the above-rated power, and shows that the air, which was expelled from the air pump, could not be condensed more in the ordinary way of working, than with a compressing power of about  $2\frac{3}{4}$  lb. on each square inch. As the engine was not regulated, at first, to make a longer stroke than about 4 feet 8 inches, only one furnace being used, the quantity of air expelled at each stroke of the machine was about 155 cubic feet, which it discharged through a valve into the air vault, about 16 times in a minute. When two furnaces afterwards were blown, the engine was regulated to work much quicker, and with a longer stroke. The air vault is 72 feet long, 14 feet wide, and 13 feet high; and contains upwards of 13,000 cubic feet, or above 80 times the  
contents

contents of the air pump. The top, sides, and bottom of this vault, where the least fissure could be discovered in the beds of the rock, were carefully caulked with oakum, and afterwards plastered, and then covered with pitch and paper. The intention of blowing into the vault is to equalize the blast, or render it uniform, which it effects more completely than any machinery ever yet contrived for the same purpose. The air is conducted from the vault by the eduction pipe, of 16 inches diameter, into an iron box or wind chest, and from this it goes off to each furnace, in two smaller pipes that terminate in nozles, or blow-pipes, of only  $2\frac{1}{2}$  to  $3\frac{1}{4}$  inch diameter, at the twee of the furnace.

WHEN the furnace was put in blast, after having been filled with coakes, and gently heated for more than six weeks, the keepers allowed it to have but little blast at first, giving it a small blow-pipe of about  $2\frac{1}{4}$  inch diameter, and likewise letting off a very considerable quantity of air, at the escape, or safety valve on the top of the iron wind chest, as it is a received though erroneous opinion among them, that the blast must be let on very gradually for several months. From the construction of this valve, it was impossible to ascertain the exact proportion of the blast they thus parted with, but I believe it was very considerable. The consequence was, that the furnace, after it had been in blast for several days, never seemed to arrive at its proper degree of heat, but was always black and cold about the twee in the hearth, and appeared in danger of choking, or gobbing, as it is termed.

AFTER various experiments tried in vain, by the keepers and the Company's engineer, and others, (indeed they tried every thing, except giving the furnace a greater quantity of air, which, as I afterwards ascertained, was all that it wanted), they concluded, that the air vault was the cause of the whole mischief; and, to confirm their opinion, they said they had

now discovered that water was, in considerable quantities, driven out of the air vault through the blow-pipe, which cooled the furnace; and they insisted, that the power of the engine was such as to force water out of the solid rock; so that this method of equalizing the blast never would succeed. The other managing partner was so much alarmed by these representations, that he began to consult with the engineer, and others, about finding a substitute for the air vault at any expence.

As the plan of the blowing apparatus had been adopted at my recommendation, and was now so loudly condemned on account of the water, I had other motives, than mere interest, for trying to become better acquainted with the phenomena attending it. I accordingly determined to go into the air vault, and to remain inclosed in the condensed air while the engine was blowing the furnace. It is an experiment that perhaps never was made before, as there never existed such an opportunity. I could not persuade the engineer, or any other of the operative people about the work, to be my companions, as they imagined that there was much danger in the experiment. Mr NEIL RYRIE, however, one of the clerks of the Devon Company, had sufficient confidence in my representations to venture himself along with me.

THE machine had been stopped about two hours previous to our entering the vault, and we found a dampness and mistiness in it, which disappeared soon after the door was shut fast upon us, and the engine began to work in its usual manner. After four or five strokes of the engine, we both experienced a singular sensation in our ears, as if they were stopped by the fingers, which continued as long as we remained in the condensed air. Our breathing was not in the least affected. I had no thermometer with me, but the temperature of the air felt to us the same as that without the vault. Sound was much magnified, as we perceived, when we talked to each other, or struck any thing;



thing ; particularly, the noise of the air escaping at the blow-pipe, or waste valve, was very loud, and seemed to return back to us. There was no appearance of wind to disturb the flame of our candles ; on the contrary, I was surprised to find, that when we put one of them into the eduction pipe, which conveys the wind from the vault to the furnaces, it was not blown out. There was not the smallest appearance of any drops of water issuing out of this pipe. The ouzing and dropping of water from the side of the rock, next the bank, seemed the same as before the condensation was made in the vault. In short, every thing appeared, in other respects, the same as when we were in the common atmosphere. Having remained about an hour in the condensed air, and satisfied ourselves that no water, during that time, that we could in the least discover, was agitated and forced out of the rock and vault by the power of the blast, as was imagined and insisted on, we gave the signal to stop the engine. As soon as it ceased to work, and the condensation abated, and before the door of the vault was unscrewed, *the whole vault, in a few seconds, became filled with a thick vapour, so that we could hardly see the candles at four or five yards distance.* The door being now opened, the work people, anxious to know our situation, and what had occurred, came into the vault, and prevented any further observations.

I NOW endeavoured to account for this curious appearance of the water, which only shewed itself occasionally, in very small quantities, at the tweer, and at a hole I ordered to be made in the bottom of the wind chest to collect it more accurately, for it never was observed, but either when the engine, after working slowly, was made to work quicker, or, after having been stopped for a few minutes, was set to work again.

I CONSIDERED the vapour which we had discovered in the vault to arise from the moisture of the side of the rock next the furnace, which being expelled by the great heat of the furnace,

and converted into vapour, was able to force its way through the pores of the rock into the vault, but that being in a manner confined within the rock, by the pressure of the condensed air, it found itself at liberty to come into the vault, only when the condensation abated considerably, or was totally removed by the going flow, or stopping of the engine. It also occurred to me, that the air, in a state of condensation, might possibly be capable of holding a greater quantity of water in solution, which might precipitate suddenly into vapour or mist when the condensation abated. I imagined, therefore, that the very small quantities of water we at times discovered, proceeded from nothing else but this vapour, in its passage to the furnace along with the blast, being condensed into water, by the coolness of the eduction pipe and iron wind chest. The quantity of water did not appear to amount to a gallon in twenty-four hours.

A FEW days after I had made this experiment, the water ceased entirely to make its appearance, either at the twee, or at the hole in the wind chest; but the furnace did not come into heat for a long while after, and indeed not till the keepers let much more air into it by a larger blow-pipe, and allowed less air to escape at the safety valve. It is probable that the rock was now become perfectly dry by the continued heat of the furnace.

My experiment had the good effect to remove all the prejudices against the plan I had adopted of blowing the furnaces, and likewise prevented the other partner from laying out a large sum of money, by stopping the works, and altering the blowing machinery. Indeed, it has since been admitted, by all who have seen it at work, to be the most simple and effective method of equalizing the blast of any yet put in practice.

THIS experiment led me, some time afterwards, to apply a wind guage that I contrived, to ascertain precisely the state of  
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the condensation of the air thrown into the furnaces. I found that a column of quick silver was raised five inches, and sometimes, though seldom, six inches, and, in the interval of the return of the engine to receive air into the air pump, it fell only half of an inch. At this time only one furnace was worked. But when two furnaces were in blast, the engine only raised the mercurial guage about 4 inches, because the Devon Company, for certain reasons, did not, while I continued a partner, think proper to allow the blowing machinery to be completed, by the putting to work their second boiler of 20 feet diameter for the fire engine, according to my original design, which, by adjusting the machinery, would have enabled us to blow two furnaces, with two boilers, with as much effect, in proportion, as one furnace with one boiler. This instrument had the advantage of enabling the work people to discover the real power of their blast, and know the exact condition of the air valves, and the gearing of the blowing piston; for if these were not tight, and in order, (although the engine might, to appearance, be doing well, by making the same number of discharges of the air pump as usual *per minute*), yet the wind guage would not rise so high, and would shew that there was an imperfection some where, by reason of a quantity of air escaping at the valves, or piston, that could not so easily otherwise be known. This contrivance was considered as of much use, and was afterwards always quoted in the Company's journal books, to shew the actual state of the blowing machine, in comparing the daily produce of the furnaces.

I HOPE you will not think me tedious, when I explain to you another experiment, which appears to me to be of considerable importance to all manufacturers of cast iron.

I HAD reason to conjecture, from my own observations on the effects of blowing machinery on blast furnaces, as well as  
from

from the knowledge I had acquired from my father Dr ROEBUCK, and from my communications with other experienced iron masters, that a great part of the power of such machinery was misapplied in general practice, by throwing air into furnaces with much greater velocity than necessary, and that, if this velocity was, to a certain degree diminished, the same power, by properly adjusting the blowing machinery, of whatever nature, would be capable of throwing into the furnace a proportionally greater quantity of air. For, “*Since the quantities of any fluid, issuing through the same aperture, are as the square roots of the pressure;*” it follows, that it would require *four times the pressure, or power, to expel double the quantity of air, through the same aperture, in the same time*: But if the area of the aperture was doubled, then the quantity of air expelled by the same power, and in the same time, would be increased in the ratio of the square root of 2 to 1, though its velocity would be diminished exactly in the same proportion. Again: I considered that the quantity and intensity of heat, produced in blast furnaces, and consequently its effects in increasing the produce, might be only in proportion to the quantity of air decomposed in the process of combustion, without regard to its greater velocity; that is to say, whether or not the same quantity of air was forced, in the same time, into the furnace through a small pipe, or through one of larger dimensions; for, in attending to the process of a common air furnace for remelting of iron, where there is a very large quantity of air admitted through the large areas between the bars, it is well known, that a much greater intensity of heat is produced than takes place in a blast furnace, and yet the air does not enter into the fire through the bars with increased density or great velocity. I therefore thought it probable, that increasing the *quantity* of air, thrown into the blast furnace in a considerable degree, although the *velocity or density* might be  
much

much less, would have the effect of increasing its heat, and operations, and produce. And as, from the principles above stated, with regard to the machinery, I saw I could greatly increase the quantity of air thrown into the furnace, by enlarging the diameter of the blow-pipe, and regulating the engine accordingly, without being obliged to employ more power, I was anxious to make this experiment.

A system of management, of which I did by no means approve, was adopted by the other partners of the Devon Company, soon after the works were begun to be erected; and, in the prosecution of it, they ordered their second furnace to be put in blast, without permitting those measures to be taken that were necessary to provide and maintain a sufficient stock of materials; and also without allowing their blowing machine to be completed, according to the original design, by the addition of its second boiler. As might have been expected, a trial of several months to carry on two furnaces, with only half the power of steam that was necessary, and an inadequate stock of materials, proving unsuccessful, the Company, as a remedy, instead of making up the above deficiencies, ordered one of the furnaces to be blown out, and stopped altogether. This improper measure, however, afforded me the opportunity of immediately putting in practice the plan I have mentioned.

WHEN one of the furnaces was stopped, the other continued to be blown by a blow-pipe of  $2\frac{3}{4}$  inches diameter, and the produce of the furnace, for several weeks thereafter, was not 20 tons of iron *per week* at an average. The engine at this time was making about 16 strokes a minute, with a stroke of the air pump, about 4 feet 8 inches long; but when I altered the diameter of the blow-pipe, first to 3, and immediately after to  $3\frac{1}{4}$  inches diameter, and regulated the working gears of the engine, so as to make a stroke of 5 feet 2 inches long, and about 19 strokes in a minute, on an average, the produce was immediately

ly increased. It continued to be, on an average of nine months immediately after this improvement, at the rate of 33 tons of iron *per week*, of as good quality as formerly; for during this period, from the 21st November 1795 to July 30. 1796, this one furnace yielded 1188 tons of iron. No more coals were consumed in working the blast engine, or other expences about the blowing machine incurred, and therefore no more power was employed to produce this great effect. It is also of much importance to remark, that the consumption of materials, from which this large produce was obtained, was by no means so great as formerly. The furnace required very considerably *less fuel, less ironstone, and less limestone*, than were employed to produce the same quantity of iron by the former method of blowing; and according to the statements made out by the Company's orders, as great a change was effected in the æconomical part of the business.

FROM the success of this experiment, so well authenticated, and continued for several months, I am led to be of opinion, that all blast furnaces, by a proper adjustment of such machinery as they are provided with, might greatly and advantageously increase their produce, by assuming this as a principle, viz. "*That with the given power it is rather by a great quantity of air thrown into the furnace, with a moderate velocity, than by a less quantity thrown in with a greater velocity, that the greatest benefit is derived, in the smelting of ironstones, in order to produce pig-iron.*" However, it is by experiment alone, perhaps, that we can be enabled to find out the exact relations of power, velocity, and quantity of air requisite to produce a *maximum* of effect\*.

BUT, an unfortunate disagreement among the partners of the Devon Company, put it out of my power to make further progress

\* If  $Q$  be the quantity of a fluid, issuing in a given time through an aperture of the diameter  $D$ ,  $V$  its velocity, and  $P$  the power by which it is forced through the aperture: then the area of that aperture being as  $D^2$ , the quantity of the fluid issuing in the given time will be as  $VD^2$ , or  $VD^2 = Q$ .

grefs in this matter, by laying me under the necessity, two years ago, of withdrawing myself entirely from the concern.

I have the honour to be, respectfully,

S I R,

Your most obedient servant,

EDINBURGH, }  
June 30. 1798. }

JOHN ROEBUCK.

To Sir James Hall, Bart.



IN order to illustrate what is said above, a ground plan of the air vault and furnaces of the Devon Iron Works is given in Plate I. ; of which the explanation follows :

EXPLANATION OF FIG. 2. PL. I.

- A The air vault, formed by a mine drove in the solid rock of coarse grained freestone.
- B The blowing cylinder.
- C The pipe that conveys the air from the blowing cylinder to the air vault.
- D The eduction pipe that carries the air from the air vault to the iron wind chest.

PART I.

F

E The

AGAIN, this quantity multiplied into its velocity, will be as the *momentum* of the fluid expelled, or as the power by which it is expelled, that is,  $V^2 D^2 = P$ , or  $VD = \sqrt{P}$ .

HERE, therefore, if  $D$  is given,  $V$  is as  $\sqrt{P}$ , as Mr ROEBUCK affirms. Also, because  $V = \frac{Q}{D^2}$ , and also  $V = \frac{\sqrt{P}}{D}$ ,  $Q = D\sqrt{P}$ , so that, while  $P$  remains the same,  $Q$  will increase as  $D$  increases, and  $V$  will diminish in the same ratio.

THE problem, therefore, of throwing the greatest quantity of air into the furnace, with a given power, strictly speaking, has no *maximum*, but the largest aperture of which the engine can admit must be the best. It is probable, however, that there is a certain velocity with which the air ought to enter into the furnace; this will produce a limitation of the problem, which, as Mr ROEBUCK suggests, is not likely to be discovered but by experiment. J. P.

- E The iron wind chest, (about  $2\frac{1}{2}$  feet cube), in which is inserted a wind-gauge, represented in fig. 3.
- F F The two blow-pipes for each furnace, which terminate in apertures of  $3\frac{1}{4}$  inches diameter at the tweers of the furnaces.
- G G The two blast furnaces, placed in two pits sunk in the solid rock.
- H H The tumps of the furnaces from whence the cast-iron is run off into the casting room, L L.
- O The door to give occasional admittance into the air vault.
- M The excavation, in which is placed the blowing machine.

## EXPLANATION OF FIG. 3.

- A The end of the wind-gauge, (about 12 inches long), which is open to the atmosphere, being half filled with quicksilver.
- B The end that is inserted in the iron wind chest, and exposed to the pressure of the condensed air of the air vault.



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III. EXPERIMENTS on WHINSTONE and LAVA. By Sir  
JAMES HALL, Bart. F. R. S. & F. A. S. EDIN.

[Read, March 5. and June 18. 1798.]

THE experiments described in this paper were suggested to me many years ago, when employed in studying the *Geological System* of the late Dr HUTTON, by the following plausible objection, to which it seems liable.

GRANITE, porphyry, and basalt, are supposed by Dr HUTTON to have flowed in a state of perfect fusion into their present position; but their internal structure, being universally rough and stony, appears to contradict this hypothesis; for the result of the fusion of earthy substances, hitherto observed in our experiments, either is glass, or possesses, in some degree, the vitreous character.

THIS objection, however, loses much of its force, when we attend to the peculiar circumstances under which, according to this theory, the action of heat was exerted. These substances, when in fusion, and long after their congelation, are supposed to have occupied a subterraneous position far below what was then the surface of the earth; and Dr HUTTON has ascribed to the modification of heat, occasioned by the pressure of

the superincumbent mafs, many important phenomena of the mineral kingdom, which he has thus reconciled to his fyftem.

ONE neceffary confequence of the pofition of thefe bodies, feems, however, to have been overlooked by Dr HUTTON himfelf: I mean, that, after their fuſion, they muſt have cooled very ſlowly; and it appeared to me probable, on that account, that, during their congelation, a cryſtallization had taken place, with more or leſs regularity, producing the ſtony and cryſtallized ſtructure, common to all unſtratiſied ſubſtances, from the large grained granite, to the fine grained and almoſt homogeneous baſalt. This conjecture derived additional probability from an accident ſimilar to thoſe formerly obſerved by Mr KEIR, which had juſt happened at Leith: a large glaſs-houſe pot, filled with green bottle glaſs in fuſion, having cooled ſlowly, its contents had loſt every character of glaſs, and had completely aſſumed the ſtony ſtructure.

THESE views made part of a paper which I had the honour of laying before this Society in 1790\*; and about the ſame time I determined to ſubmit my opinions to the teſt of experiment. I communicated this intention to all my friends, and in particular to Dr HUTTON; from him, however, I received but little encouragement. He was impreſſed with the idea, that the heat to which the mineral kingdom has been expoſed was of ſuch intensity; as to lie far beyond the reach of our imitation, and that the operations of nature were performed on ſo great a ſcale, compared to that of our experiments, that no inference could properly be drawn  
from

\* PARTICULAR reaſons induced me not to publiſh this paper at full length; but, wiſhing to preſerve a record of ſome opinions peculiar to myſelf which it contained, I introduced a ſhort abſtract of it into the Hiſtory of the Tranſactions.

from the one to the other. He has since expressed the same sentiments in one of his late publications, (*Theory of the Earth*, vol. I. p. 251.), where he censures those who “judge of the “great operations of the mineral kingdom, from having kindled a fire, and looked into the bottom of a little crucible.”

BUT, notwithstanding my veneration for Dr HUTTON, I could not help differing from him on this occasion: For, granting that these substances, when in fusion, were acted upon by a heat of ever so great intensity, it is certain, nevertheless, that many of them must have congealed in moderate temperatures, since many are easily fusible in our furnaces; for it is impossible that a substance should congeal at a higher point than that at which it may afterwards be melted. If, then, these phenomena depend upon the circumstances of congelation, the imitation of the natural process is an object which may be pursued with rational expectation of success; and, could we succeed in a few examples on a small scale, and with easily fusible substances, we should be entitled to extend the theory, by analogy, to such as, by their bulk, or by the refractory nature of their composition, could not be subjected to our experiments. It is thus that the astronomer, by observing the effects of gravitation on a little pendulum, is enabled to estimate the influence of that principle on the heavenly bodies, and thus to extend the range of accurate science to the extreme limits of the solar system.

ENCOURAGED by this reasoning, I began my projected series of experiments in the course of the same year (1790), with very promising appearances of success. I found that I could command the result which had occurred accidentally at the glass-house; for, by means of slow cooling, I converted bottle glass, after fusion, into a stony substance, which again, by the application of strong heat, and subsequent rapid cooling, I restored to the state of perfect glass. This operation I performed repeatedly with the same specimen, so as to ascertain that the character

rafter of the result was stony or vitreous, according to the mode of its cooling.

SOME peculiar circumstances interrupted the prosecution of these experiments till last winter, when I determined to resume them. Deliberating on the substance most proper to submit to experiment on this occasion, I was decided by the advice of Dr HOPE \*, well known by his discovery of the Earth of Strontites, to give the preference to whinstone.

THE term whinstone, as used in most parts of Scotland, denotes a numerous class of stones, distinguished in other countries by the names of basalt, trap, wack, grüstein and porphyry. As they are, in my opinion, mere varieties of the same class, I conceive that they ought to be connected by some common name, and have made use of this, already familiar to us, and which seems liable to no objection, since it is not confined to any particular species †.

THE following experiments were performed with various kinds of whinstone, and have likewise been extended to lava. To investigate the relation between these two classes of substances, seems, in the present state of geology, an object of considerable importance; for they resemble each other in so many respects, that we are naturally led to ascribe the formation of both to the same

\* IN the course of last winter, when I first thought of resuming my experiments, I proposed to this gentleman, that, in imitation of a practice, common in the Academy of Sciences of Paris, we should perform them in company. To this proposal he cheerfully agreed; but, before any experiments had been begun, he found himself so much occupied by professional duties, that he could not bestow upon the subject the time which it necessarily required; and we gave up the idea of working in company.

† IN characterising the particular specimens, I have adopted, with scarcely any variation, descriptions drawn up by Dr KENNEDY, whose name I shall have occasion frequently to mention in the course of this paper. In the employment of terms, we have profited by the advice of Mr DERIABIN, a gentleman well versed in the language of the Wernerian School.

same cause, and to believe that whinstone, as well as lava, has been exposed to the action of heat. In the course of the paper, I shall mention several accidental results, which, if considered separately, might seem unworthy of notice, but which, by affording the means of comparison between the two classes, are of great service in the general investigation.

THE whinstone first employed was taken from a quarry\* near the Dean, on the Water of Leith, in the neighbourhood of Edinburgh. This stone is an aggregate of black and greenish-black hornblend, intimately mixed with a pale reddish-brown matter, which has some resemblance to felspar, but is far more fusible. Both substances are imperfectly and confusedly crystallized in minute grains. The hornblend is in the greatest proportion; and its fracture appears to be striated, though in some parts foliated; that of the reddish-brown matter is foliated. The fracture of the stone *en masse* is uneven, and it abounds in small facettes, which have some degree of lustre. It may be scratched, though with difficulty, by a knife, and gives an earthy smell when breathed on. It frequently contains small specks of pyrites.

ON the 17th of January 1798, I introduced a black lead crucible, filled with fragments of this stone, into the great reverberating furnace at Mr BARKER's iron foundery. In about a quarter of an hour, I found that the substance had entered into fusion, and was agitated by a strong ebullition. I removed the crucible, and allowed it to cool rapidly. The result was a black glass, with a tolerably clean fracture, interrupted however by some specks.

IN subsequent experiments, I endeavoured, by slow cooling after fusion, to prevent the whinstone from becoming vitreous, and to compel it to resume its original character by crystallization. In this I so far succeeded as to obtain a substance, which was not glass, though it did not possess the properties

of

\* CALLED Bell's Mills Quarry.

of whinstone. The production of this intermediate substance, which much resembled the liver of an animal, is accompanied with some curious particulars, which I shall enumerate and explain in another part of this paper. On some occasions, too, I obtained a vitreous mass in which were a multitude of little spheres, having a dull or earthy fracture.

At last, on the 27th of January, I succeeded completely in the object I had in view. A crucible, containing a quantity of whinstone, melted in the manner above described, being removed from the reverberatory, and conveyed rapidly to a large open fire, was immediately surrounded with burning coals, and the fire, after being maintained several hours, was allowed to go out. The crucible, when cold, was broken, and was found to contain a substance, differing in all respects from glass, and in texture completely resembling whinstone. Its fracture was rough, stony and crystalline; and a number of shining facets were interspersed through the whole mass. The crystallization was still more apparent in cavities produced by air bubbles, the internal surface of which was lined with distinct crystals\*.

HAVING shewn this result to several of my friends, Dr HOPE regretted that the substance, previously to its artificial crystallization, had not been reduced to the state of solid glass; since the adversaries of the system might allege, that, during the action of heat, the original crystallized texture of the stone had never been completely destroyed. Being convinced of the propriety of this observation, I determined, in future, to reduce the stone first to glass, and to perform the crystallization after a second fusion.

FOR this purpose, with the assistance of Dr KENNEDY, to whose co-operation I am greatly indebted for the success of all the following experiments, I reduced a quantity of the same whinstone to

\* I SHOWED this result at a meeting of the Society on 5th of February.

to most perfect black glass. A crucible, filled with fragments of this glass, being then exposed to a heat, which, from previous trials, was judged to be more than sufficient to reduce its contents to fusion, the fire was very gradually lowered till all was cold. I thus expected to obtain a result similar to that last mentioned, but found, to my great surprise, that the fragments had never been in complete fusion; since they still, in a great measure, retained their original shape. This extraordinary fact, which afterwards led to the discovery of some curious properties of whinstone, will be fully accounted for in a subsequent part of the paper.

ANOTHER portion of the same glass being perfectly melted by a very strong heat, the temperature was reduced to about 28 of WEDGWOOD, and was maintained at that pitch during six hours. The result was a perfectly solid mass, crystallized to a certain depth from the outside, though still vitreous in the heart. In another experiment, performed like the last in all respects, except that the heat was maintained at 28 during twelve hours, I obtained a mass entirely crystalline and stony throughout, with facettes appearing in the solid parts, and small crystals shooting into some of the cavities.

SOON after I had communicated these results to Dr HOPE, he performed, with complete success, an experiment similar to the first, in which I had obtained a crystallized substance, by the gradual cooling of the melted stone. The same was likewise, soon afterwards, performed by Mr BOSWELL of Auchinleck.

MY experiments, already described, were confined to one species of whinstone; but have since been extended to six other varieties. They were all first reduced to glass by the application of a strong heat, and subsequent rapid cooling. After a second fusion they were crystallized, by being kept long in a stationary temperature, between 28 and 30. This last operation was best performed in a long and narrow muffle, wholly surrounded

ed with burning coals, according to a practice long followed by Dr KENNEDY, by which the heat could be maintained with so great steadiness as to render the result almost certain.

THE fusibilities were determined in an open muffle, in which a fragment of the substance under trial was placed contiguous to a pyrometer piece. As soon as the fragment, in consequence of the gradual rise of heat, had so far softened as to yield to the touch of a bent iron rod, the pyrometer was removed and measured. The fusibilities, thus obtained, in degrees of WEDGWOOD'S scale \*, have been stated in a table, to which I would be understood always to refer. I have distinguished the crystallized substances, obtained from the glasses, by the name of *crystallite*, a term suggested by Dr HOPE. It may be observed in this table, that the original whins soften in a range from 38 to 55; the glasses from 15 to 24, and the artificial crystallites from 32 to 45.

#### No. 1. *Whin of Bell's Mills Quarry.*

THIS stone was the subject of all the foregoing experiments, which were frequently repeated with success on a large scale.

IN trying the fusibility of the glass obtained from it, a curious circumstance occurred, which accounts for the unexpected results

\* THE measurement of the temperatures may be relied upon as accurate; they were determined by two sets of pieces, one purchased by me during the lifetime of the late Mr WEDGWOOD, and the other likewise made by him, belonging to Dr KENNEDY. The two sets correspond exactly; and Dr KENNEDY'S had, at his request, been carefully examined by the present Mr WEDGWOOD, who found them true by his father's original standard.



results already mentioned. I had placed in the muffle a long and slender fragment of this glass, with its extremities resting on two supports of clay, and its middle unsupported. Having then increased the temperature by slow degrees, I expected to discover the lowest point of emollescence, by observing when the fragment sunk by its own weight. The muffle having attained a moderate heat, I observed the glass to lose its shape a little. Wishing to see it completely melted, the same heat was continued, but no further change took place. The heat was then raised several degrees, but without effect. At last, being urged still further, the glass sunk down completely between its supports. The pyrometer being then withdrawn, denoted a temperature above 30.

It occurred to me, that, on this occasion, the glass, by the first application of heat, had softened, and then had crystallized, so as to become hard again; that, in crystallizing, it had acquired such infusibility as to yield to no heat under 30. I immediately confirmed this conjecture by the following experiment.

A PIECE of the same glass, placed in a cup of clay, was introduced into the muffle, heated to 21. In one minute it became quite soft, so as to yield readily to the pressure of an iron rod. After a second minute had elapsed, the fragment, being touched by the rod, was found to be quite hard, though the temperature had remained stationary. The substance, thus hardened, had undergone a change throughout; it had lost the vitreous character; when broken, it exhibited a fracture like that of porcelain, with little lustre; and its colour was changed from black to dark brown. Being exposed to heat, it was found to be fusible only at 31; that is, it was less fusible than the glass by 13 or 14 degrees.

Numerous and varied experiments have since proved, in the clearest manner, that, in any temperature, from 21 to 28 inclu-

five, the glass of this whin passes from a soft, or liquid state, to a solid one, in consequence of crystallization; which is differently performed at different points of this range. In the lower points, as at 23, it is rapid and imperfect; in higher points, slower and more complete, every intermediate temperature affording an intermediate result. I likewise found, that crystallization takes place, not only when the heat is stationary, but likewise when rising or sinking, provided its progress through the range just mentioned is not too rapid. Thus, if the heat of the substance, after fusion, exceeds one minute in passing from 21 to 23, or from 23 to 21, the mass will infallibly crystallize, and lose its vitreous character.

THESE facts enabled me to account for the production of the substance resembling the liver of an animal, which I obtained in my first attempts to crystallize the melted stone. Not being then aware of the temperature proper for complete crystallization, I had allowed it to be passed over rapidly by the descending heat, and I had begun the slow cooling in those lower points, at which the formation of this intermediate substance takes place.

By the same means I was enabled to explain the other unexpected result, which I obtained in endeavouring to convert the glass of this stone into crystallite. The fire applied to the crucible, containing fragments of the glass, had been raised very slowly, which I know to have been the case by some circumstances of the experiment. The glass had softened by the first application of heat, but had crystallized again as the heat gradually rose; so that the substance consolidated, while still so viscid as to retain the original shape of the fragments; at the same time it acquired such infusibility as to resist the application of higher degrees of heat during the rest of the process.

No. 2. *Whin of the Rock of Edinburgh Castle.*

THIS is a basalt of a blackish blue colour. Its grain is fine, and its fracture uneven, partaking of the splintery. It is in general homogeneous, although, in some pieces, a very few minute crystals of hornblend are perceptible. It has some lustre, from a number of small shining facettes; has an earthy smell when breathed on; and gives fire slightly with steel.

THE pure glass which this whin yielded, by rapid cooling after a moderate heat, was crystallized in three experiments, and produced masses greatly resembling the original. In one of these, formed on a large scale in the glass-house, the resemblance is so strong, both as to colour and texture, that it would be difficult, or perhaps impossible, to distinguish them, but for a few minute air bubbles visible in the artificial crystallite. The glass is less fusible than that of No. 1. and seems not to possess the property of producing the liver crystallite.

No. 3. *Whin of the Basaltic Columns on Arthur's Seat, near Edinburgh.*

ITS basis is a basalt of a dark grey colour, and uneven fracture. It contains numerous laminar crystals of felspar, which seem to be almost colourless, and have considerable lustre and transparency. It also contains some black hornblend. It has an earthy smell when breathed on, and gives sparks slightly with steel.

IN the temperature of 100, or upwards, the whole was changed to pure black glass; but in a more moderate heat, (about 60), the felspar remained unchanged, while the hornblend disappeared, and formed a glass along with the basis of the stone.

Both

Both kinds of glass yielded highly characterised crystallites; that last mentioned, having its felspars entire, produced a substance like porphyry, in which the white felspars were embedded in a black crystalline basis. The crystals formed in this basis are so complete in one example, that they are seen projecting into the cavities, and standing erect on the external surface, so as to make it sparkle all over. These black crystals seem to be hornblend of new formation. We have found, by some late experiments, that they are considerably more refractory than the crystallite in which they lie, and are equally infusible with some species of natural hornblend.

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No. 4. *Whin from the neighbourhood of Duddingstone Loch.*

It has for its basis a black basalt of an uneven fracture. In it are embedded augit in numerous crystals, felspar in a smaller proportion, and dispersed grains of olivin. The felspar seems to be greenish-white, with considerable lustre and transparency. The stone gives fire with steel, and has a slight earthy smell when breathed on. Its glass yields a fine grained crystallite, like that of No. 1.

No. 5. *Whin of Salisbury Craig near Edinburgh.*

This species is an aggregate of black hornblend, and of a greenish-white matter, both in minute grains. The greenish-white matter resembles felspar, but is much more fusible. The general characters are nearly the same with those of the specimen already described, No. 1. It has considerable lustre, chiefly from the hornblend; an earthy smell when breathed on; and gives some sparks with steel. Its glass yielded a highly faceted crystallite,

crystallite, approaching to the structure of the original whinstone, No. 4.

No. 6. *Whin from the Water of Leith.*

It is found in great blocks in the bed of the river, and has been brought there no doubt from a mass of the same kind in the mountains above. It consists of black hornblend, and of a whitish matter resembling felspar, as in No. 1. and No. 5. These two substances are nearly in equal proportion, and are confusedly and imperfectly crystallized in minute masses. If the whitish substance were felspar, this stone, as well as that last mentioned, would be the grüenstein of Werner; but this white substance is far more fusible than felspar, and melts at a lower heat than the hornblend, with which it is mixed. It has an earthy smell when breathed on, and may be scratched with difficulty by a knife.

In fusion and crystallization it resembled the other whins. A fragment similar to this in all respects, which I found in the neighbourhood of Edinburgh, manifested so strong a disposition to crystallize, that, though cooled in the open air after fusion, it was found stony in the heart, with a vitreous outside. When crystallized, however, with every precaution, it yielded no remarkable result.

No. 7. *Whin of the Basaltic Columns of Staffa.*

I RECEIVED this specimen from a gentleman who broke it from the original rock. It is basalt of a bluish-black colour. It is fine grained and homogeneous; and its fracture is uneven. It has a small degree of lustre, from a number of minute shining points perceptible in a strong light. It gives an earthy smell when breathed on, and may be scratched with difficulty by a knife. It yielded a perfect and very hard glass, which, in a regulated heat,  
produced

produced a uniform stony crytallite, greatly resembling the original.

It has thus been shown, that all the whins employed assume, after fusion, a stony character, in consequence of slow cooling ; and the success of these experiments, with so many varieties, entitles us to ascribe the same property to the whole class. The arguments, therefore, against the subterraneous fusion of whinstone, derived from its stony character, seem now to be fully refuted.

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*Experiments on Lava.*

IN the investigation of Dr HUTTON's system, great advantage may be expected from an examination of lavas. They have undoubtedly flowed on the surface by means of heat ; and whinstone, according to his hypothesis, having flowed in the bowels of the earth by the influence of the same agent, the two classes ought to possess many properties in common, by which the history of both may be illustrated.

I HAVE been enabled to institute a comparison between them, by means of a cabinet of volcanic productions which I collected in 1785, in company with Dr J. HOME of this Society, on Vesuvius, Ætna, and the Lipari Isles. On this occasion we were greatly assisted by the celebrated M. DOLOMIEU \*, who accompanied us in part of our expedition. This author complains, in his writings, that travellers, in collecting volcanic productions, have brought away only the superficial scoria of lavas, which nearly resemble each other in all cases, and convey  
no

\* THOUGH I differ widely from this gentleman in many of his theoretical opinions, I cannot too strongly express my admiration of his merit as a natural historian. His descriptions of countries, as well as of minerals, present the most lively representations to the mind of the reader, which, in the numerous instances I have witnessed, are perfectly correct.

no idea of the real character of the lava, which can only be seen in the interior parts of the currents. In forming our collection we scrupulously avoided this error, and chose such specimens only as were the most compact and free from the scorified appearance of the surface.

WHEN these solid lavas are compared with our whinstones, the resemblance between the two classes is not only striking at first sight, but bears the closest examination. They both consist of a stony basis, which frequently contains detached crystals of various substances, such as white felspar and black hornblend. The analogy between the two classes seems to hold through all their varieties; and I am confident that there is not a lava of Mount *Ætna* to which a counterpart may not be produced from the whinstones of Scotland.

THIS resemblance in external character is accompanied with an agreement no less complete in chemical properties. But before I mention the experiments which tend to prove this agreement, it will be necessary first to examine the opinion of two very celebrated authors concerning lavas. M. DOLOMIEU and Mr KIRWAN, though they differ widely in many respects, agree in believing, that lavas have never been acted upon by a heat of sufficient intensity to produce complete fusion; and endeavour, each by an hypothesis peculiar to himself, to account for their fluidity. The opinion of these gentlemen is of such importance in the present question, and the arguments they have used are so extraordinary, that I must beg leave to quote their words at full length.

M. DOLOMIEU states his opinion in the following passage, (*Isles Ponces*, p. 7.): “ Il est essentiel de constater, par beau-  
 “ coup d'exemples et d'observations, quelques vérités que j'ai  
 “ annoncées il y a plusieurs années, savoir, que le feu des vol-  
 “ cans ne dénature pas ordinairement les pierres qu'il a mises  
 “ en état de fusion; qu'il ne les altere pas au point de ne pou-  
 “ voir les reconnoître, de ne pas distinguer quelle a pu être la  
 PART I. H “ base

“ bafe des laves ; que ce feu agit différemment que le feu de  
 “ nos fourneaux, tel que nous l’employons dans la chimie et  
 “ dans les arts ; qu’il produit dans les laves une fluidité qui  
 “ n’a aucun rapport avec la fluidité vitreufe, que nous opérons,  
 “ lorsque nous traitons à grand feu les mêmes matières qui leur  
 “ fervent de bafe, et lorsque nous voulons rendre aux laves elles  
 “ mêmes leur fluidité. Celui des volcans n’a point d’intensité ;  
 “ il ne peut pas même vitrifier les fubftances les plus fufibles,  
 “ tels que les fchorls, qui fe trouvent comme parties confti-  
 “ tuantes dans l’intérieur des laves ; il produit la fluidité par  
 “ une efpece de diffolution, par une fimple dilation, qui permet  
 “ aux parties de gliffer les unes fur les autres, et peut-être encore  
 “ par le concours d’une autre matière qui fert de véhicule à la  
 “ fluidité.”

MR KIRWAN cenfures this fuppoftion as ftrange and incon-  
 ceivable ; but in my opinion, that which he has brought for-  
 ward is not lefs fo. In the *Elements of Mineralogy, fecond edition,*  
*vol. I. p. 396.* he fays : “ Now, there are but three forts of fu-  
 “ fion with which we are acquainted : that which produces por-  
 “ celain ; that which produces enamels and femi-vitrifications ;  
 “ and that which produces glafs. By infpecting lava we fhall  
 “ find that very little of it has been in any of thefe ftates ; fince  
 “ therefore it has flowed, it is plain it has derived its liquifac-  
 “ tion not from the fufion of its own materials, but from that  
 “ of fome foreign fubftance mixed with it. This fact is fo plain,  
 “ that it has even ftruck M. DOLOMIEU, in the midft of his  
 “ prepoftleffions, in favour of fome ftrange inconceivable power,  
 “ which he attributes to volcanic fire, of melting earthy fub-  
 “ ftances, without effecting an alteration in their fenfible quali-  
 “ ties. “ I hope,” fays he, “ to prove, that lavas contain, in  
 “ their interior, a combuftible matter, which burns and con-  
 “ fumes in the fame manner as other inflammables,” *Ifles Pon-*  
 “ *ces*, 10. Yet he neglects telling us what this matter is ; though  
 “ it plainly appears to be no other than fulphur and bitumen,  
 “ of



“ of which an immense quantity is found in all volcanos,  
 “ which liquifies in a low degree of heat, and causes all the  
 “ stony substances to flow that are immersed in it.”

THE suppositions which these gentlemen have thus advanced, and have seriously maintained in various parts of their works, have arisen in both from the belief, that, in our fires, nothing but glass can be produced from a lava after complete fusion. This being taken for granted, it would certainly be very difficult to explain the phenomena of actual eruptions, by means of the known agents of nature. Recourse has therefore been had, by one of these gentlemen, to a hypothetical modification of these agents; and by the other to the influence of substances, which have left behind them no trace of their existence\*, and which, had they been present, could not have produced the effects ascribed to them.

ACCORDING to both suppositions, the heat of volcanos is conceived to be of very little intensity; but the few observations I had occasion to make, which are confirmed by innumerable facts related by travellers, convince me that it must far exceed what is requisite for the most perfect fusion of the lavas, and of all the substances contained in them†; and the experiments already described supersede the necessity of supposing any thing different from the common course of nature; for they afford, analogically, an easy solution of the difficulty, by showing that glass is not the only result of fusion, and that when, a substance like lava, when cooled slowly after fusion,

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resumes

\* NONE of the lavas I have seen contained the smallest vestige of petroleum; nor did I meet with any sulphur but what was evidently produced by the condensation of vapours, rising through crevices, long after the eruptions had ceased.

† I CONCEIVE, therefore, that the formation of the insulated substances contained in lavas, as well as the other peculiarities of internal structure, possessed by lavas in common with granite and basaltes, must be ascribed in all of them to crystallization during slow cooling after fusion, as I stated formerly in Spring 1790, (*Transf. Edin. vol. III.*). The year following, Dr BEDDOES presented to the Royal Society of London a paper, in which he also explains the character of granite and basaltes by crystallization, in consequence of slow cooling.

resumes its stony character. But, not content with analogy alone, I resolved to ascertain the truth of these conclusions in a direct manner, and performed the following experiments with specimens of six different lavas, four of which, to my certain knowledge, had made part of external volcanic currents. In the present state of geology, too much pains cannot be bestowed in ascertaining that the specimens collected are really lavas, since this circumstance has been frequently overlooked, as I shall endeavour to shew, when I speak of the differences between them and whinstone.

#### No. 1. *Lava of Catania.*

THIS is the celebrated lava, which, in 1669, laid waste great part of the town of Catania. The interior part of the current, (accurately described by M. DOLOMIEU, *Iles Ponces*, p. 256.\*), from which the subject of our experiment was taken, consists of a light grey basis, interspersed with crystals of felspar and of schorl, (augit). It bears a general resemblance to the rock of the basaltic columns on Arthur's Seat, and exhibited the same phenomena in our experiments. After strong heat, the whole was reduced, by rapid cooling, to pure black glass; but when the heat applied was moderate, the felspars remained unchanged. Being maintained, after a second fusion, in a temperature of 28, both these glasses yielded stony and crystallized substances, somewhat less fusible than the original; and when

\* "ELLE est formée d'une pâte de roche de corne grise, à grains fins, mêlée d'écailles, et de cristaux de feld-spath de même couleur; elle contient un très grand nombre de cristaux de schorl noir, et de grains de crysolites jaunes, les uns et les autres quelquefois chatoyans, de différentes couleurs dans leurs fractures. — — — — — Cette lave a une cassure sèche, et un grain rude, surtout dans le centre des courans; c'est là où elle a toujours conservé une couleur plus claire, qui doit être celle de sa base; sur les bords et les surfaces elle s'est fort noircie; elle y a acquis une assez forte action sur l'aiguille aimantée que celle du centre n'a presque point."

when exposed to a temperature of 22, they crystallized rapidly, like most of the whins, into the liver crystallite. This last property is common to all the lavas.

No. 2. *Lava of Sta Venere.*

THIS current has flowed in the neighbourhood of a little chapel, called Sta Venere, above the village of Piedimonte, on the north side of Mount Ætna. Owing to the strong resemblance which it bears to stones supposed not volcanic, we took care that our specimens should be broken from the actual current; and to one of them, though mostly compact, is attached a scorified mass, which had made part of the external surface. The solid part is of a black, or rather dark blue, colour, very fine grained and homogeneous, having a multitude of minute and shining facettes visible in the sun; in this, and in other circumstances, it greatly resembles the rock of Edinburgh Castle. This lava is the second in M. DOLOMIEU's *Catalogue*, and is well described, p. 186\*.

THE pure black glass formed from this lava yielded, in the regulated heat, the most highly crystallized mass we have obtained from any lava or whin.

No. 3. *Lava of La Motta di Catania.*

THIS is likewise compact and homogeneous, but for a number of small yellow grains of chrysolite scattered through it, (described by M. DOLOMIEU, p. 191†). It has been thrown up by a partial eruption bursting through the sandstone hills which

\*“LAVE homogène noire : son grain est fin et serré, il est un peu brillant, comme micacé lorsqu'on le présente au soleil ; sa cassure nette et sèche est conchéroïde comme celle du filix.”

† It belongs to the fifth variety of his compact lavas.

which surround Mount Ætna. The situation of this mass is singular: It rests upon a little hill, formed of loose scoria, the summit and sides of which are covered by the stony mass, so that no crater is visible. It struck me on seeing it, and I found M. DOLOMIEU had formed the same opinion, that the lava had risen up in a perpendicular direction, and had flowed over on all sides. Its great thickness, and small extent, seem to favour a conjecture which this naturalist has formed with regard to several lavas, that they were erupted at the bottom of an ocean which once covered Sicily, and, being quickly cooled by the contact of water, had been prevented from flowing far. The conjecture seems plausible enough \*; and, having no proof that this substance made part of an external current, as I have with respect to the first two mentioned, I do not exhibit it as a lava with the same confidence. Whatever be its history, however, it possesses the chemical properties common to whin and lavas.

Its glass yielded a dark grey crystallite of uniform texture. Beside it in the drawer, now on the table, I have placed a crystallite, formed from the whin No. 1. which resembles it in every respect.

#### No. 4. *Lava of Iceland.*

I RECEIVED the specimen from a person who found it on the spot; but not being acquainted with the circumstances of its original position, I cannot be certain that it is a lava. It has however every appearance of being such.

It is a blue homogeneous substance, having some chrysolites scattered irregularly through it. Nearly half its bulk is occupied

\* M. DOLOMIEU ascribes the formation of part of Mount Ætna itself to a similar cause. I shall have occasion, in another part of this paper, to consider that opinion.

pied by large air holes, which do not appear to have contained any extraneous matter.

It produced a very fusible glass, from which was formed a crystallite much more refractory than the original.

No. 5. *Lava of Torre del Greco.*

THIS lava, which flowed from Vesuvius to the sea in the middle ages, has been an object of much attention, on account of its conspicuous basaltic form. It consists of a grey basis, the fracture of which is coarse and rough, and in which are embedded large and well characterized crystals of schorl, (augit), with a few chrysolites, (olivins).

It was found to be less fusible than any of the others, yet its glass crystallized in a lower temperature.

No. 6. *Lava of Vesuvius, eruption 1785.*

FROM the circumstances in which the above five lavas have been seen to crystallize after fusion, it can scarcely be doubted that the same process takes place in a volcanic stream; which, in consequence of its bulk, must cool with considerable slowness, and that a vitreous character would be assumed by the whole mass, were it cooled with sufficient rapidity.

THE truth of this last opinion is demonstrated by some facts which I accidentally observed, long before my present views had occurred, when, in spring 1785, I had an opportunity of examining a stream of lava, which flowed from Vesuvius. The eruption was comparatively so gentle, that I was able, though not without inconvenience, to approach and examine the fiery stream.

stream on three different days. It was in general concealed by a thin white smoke, which the wind blew aside occasionally, so that I could distinctly see the lava as it burst from the side of the hill. It was then of a bright white heat, and flowed with the agility and rapidity of water, in all respects resembling melted iron running from the furnace. The liquid, at its first emergence, manifested a strong effervescence, which subsiding as the heat abated, shewed itself at last only in the bursting of some very large bubbles, accompanied with a white smoke. Where I approached the stream, it was still of a strong red heat, and had the consistence of honey. I thrust a stick into it with ease, to the end of which some of the lava adhering, by its viscidty, allowed itself to be drawn out into threads, and was found, when cold, to have a shining surface, and a vitreous fracture.

BEING thus convinced that I had met with a lava of glass, I prepared some moulds of stucco, in which I meant to take casts with that rare substance; and with this view returned to the mountain. I found the stream was not so liquid as at first, but I was able, by means of a ladle fixed on the end of a pole, to lift the specimen now before us in a state like dough. I then pressed it with a seal, by which means, though too coarse to receive an accurate impression, it took the shape it now bears, which is that of the ladle. It is very porous, one-third of it nearly being occupied by air holes. It contains a great number of small white crystals of Vesuvian garnet, embedded in a black substance, which completely resembles the glass obtained in our experiments from lava by rapid cooling after fusion. Besides all their other properties, it possesses the fusibility of the glasses, since it softens completely at 18, that is, 14 or 15 degrees below the softening point of any of the stony lavas. Being exposed to the process of regulated cooling, it gave the same result as all the other lava glasses. In the lower  
points

points it yielded a liver crytallite infusible under 30, and in the higher a stony substance like a common lava or whin, and fusible only at 35.

WHAT has been said is applicable to the interior parts of lavas; but I was at a loss to understand the state of their external surface, which, cooling much more rapidly, might be expected to possess a vitreous character; yet glass is not found on the surface of lavas, except in a very few cases, and has occurred only in a single spot on *Ætna*. This difficulty was removed, however, by the following consideration: Though the surface of a lava cools with far more rapidity than the rest of the mass, yet, owing to the contact of the fiery stream, that rapidity can never be very great; and we must suppose that the temperature of the surface employs more than a minute or two in descending from 23 to 21. Where this happens, we have shown that the substance consolidates into the liver crytallite, which completely resembles the scoria of a lava. A small fragment of the mass, which I took from the running stream, being placed in the temperature of 22, lost its vitreous character in two minutes, as already stated; and had the mass itself been allowed to remain but a very little longer in the stream, it would certainly have acquired, as well as the rest of the surface, the dull character of scoria.

THE same property accounts for the crust which is formed on the surface of flowing lavas, and which constitutes so remarkable a feature in their history. Were lava to congeal after the manner of pitch or wax, by an uniform and gradual increase of viscosity throughout, no crust would be formed, or if, by the action of cold air, the upper surface were to harden a little, it might be softened again by an influx of fresh matter a very little hotter than itself. In lavas, however, as we have proved, when the surface cools down to 21, it rapidly congeals to a hard substance, capable of resisting any heat under 30. The

crust thus formed serves as a pipe, within which the flowing lava is confined. In several places on *Ætna* we meet with vast galleries, along which, and out of which, the lava has flowed, leaving the crust entire \*.

THE irregular manner in which a lava flows, when not extremely heated, may likewise be referred to the same cause. On the lower part of the running stream a crust is formed, so strong as to retard its progress during a certain time, but the liquid behind, accumulating by degrees, at last acquires sufficient strength to force open the crust; the lava then flows out with rapidity, and continues its course till it is again retarded by the formation of a new crust.

THESE experiments seem to establish, in a direct manner, what I had deduced, analogically, from the properties of whinstone, namely, that the stony character of a lava is fully accounted for by slow cooling after the most perfect fusion; and, consequently, that no argument against the intensity of volcanic fire can be founded upon that character. We are therefore justified in believing, as numberless facts indicate, that volcanic heat has often been of excessive intensity.

IN the comparison instituted between whin and lava, the two classes are found to agree so exactly in all their properties which we have examined, as to lead to a belief of their absolute identity. This identity has been fully established by Dr KENNEDY, who has performed an exact analysis † of several of the very specimens of whinstone and lava mentioned in this paper; by which he discovered, that the elements of the two classes are the same: above all, that they both contain 4 or 5 *per cent.* of soda. Their agreement in this essential circumstance seems to account for their common properties, whilst the varieties of proportion, among their component elements, correspond to the slight differences

\* As at Malpertui above Piedimonte.

† AN account of Dr KENNEDY's analysis is published in this volume.



ferences of result we have observed between the individuals of the same class \*.

So close a resemblance affords a very strong presumption in favour of Dr HUTTON's system, according to which both classes are supposed to have flowed by the action of heat ; but the circumstances under which they were exposed to this action being materially different, we have reason to look for indications of that difference. Such are not wanting.

CALCAREOUS spar frequently occurs in whinstone, either in veins or in detached nodules, but is never found in lava, and could not exist in a volcanic stream ; for heat, in such circumstances, would infallibly drive off the carbonic acid, and compel the lime to unite with the other component elements of the mass. In whinstone, which Dr HUTTON supposes to have flowed, at some remote period, in crevices of the earth, at a great depth below what was then its surface, the weight and strength of the superincumbent mass of strata † has been sufficient to

I 2

resist

\* THOUGH chemists have hitherto overlooked, in their experiments, the mode in which bodies were cooled after being reduced to a state of fusion ; yet many results, which we are now entitled to ascribe to slow cooling, have been occasionally observed. The slag of a furnace bears a strong resemblance to what we have called the liver crystallite, and is probably formed in the same manner. I have seen a mass possessing, in a great measure, the stony character of whins and lavas, which was produced in a lime-kiln by the fusion of an impure limestone ; and Dr BEDDOES has observed a crystallized texture in the slags of some iron furnaces. I am informed, that the celebrated Mr KLAPROTH has described some striking examples of crystallization after fusion, which he obtained in exposing various substances to the heat of the porcelain furnace at Berlin.

† It may be asked, what has become of this superincumbent mass ; and by what means it has been removed. Dr HUTTON answers, that it has been gradually worn away during an immense course of ages, by the action of those causes which continue, under our eyes, to corrode the surface of the globe : That the solid parts, being conveyed to the bottom of the ocean, are there deposited in beds of sand and gravel, which, in some future revolution, being exposed to heat, may be again converted into stony strata.

THE

resist the expansion of the carbonic acid, and to constrain it, upon the principle of PAPINS digester, to continue in combination with the lime. This compound seems to have entered readily into fusion, along with the whinstone, but to have kept separate from it, as oil separates from water through which it has been diffused, thus giving rise to the spherical form, which the nodules of calcareous spar generally exhibit with more or less regularity\*.

THIS circumstance accounts for an appearance which has misled some of the early observers of our minerals. Many whinstone

THE whole of this system appears to me well founded, except in what regards the removal of the superincumbent mass, which has been performed, I conceive, in a very different manner. I am inclined to agree on this point with M. PALLAS, M. DE SAUSSURE, and M. DOLOMIEU, and to believe that, at some period very remote with respect to our histories, though subsequent to the induration of the mineral kingdom, the surface of the globe has been swept by vast torrents, flowing with great rapidity, and so deep as to overtop the mountains; that these torrents, by removing and undermining the strata in some places, and by forming in others immense deposits, have produced the broken and motley structure, which the loose and external part of our globe every where exhibits.

IN the Alps and in Sicily I have witnessed several of those curious facts, upon which M. DE SAUSSURE and M. DOLOMIEU found their opinion, and which seem to justify their conclusions. I have likewise observed, in this country, many phenomena which denote the influence of similar agents. Lord DAAK, who joins me in agreeing with Dr HUTTON in almost every article but this, has added great weight to the argument by some general observations on lakes, and by some very interesting facts which he has observed in the Highlands of Scotland. We propose to pursue this subject, and to lay the result of our inquiries before the Society. Dr HUTTON, in the second volume of his *Theory of the Earth*, has taken great pains to refute all that has been said about these torrents; but, in my opinion, their existence is not only quite consistent with his general views, but seems deducible from his suppositions, almost as a necessary consequence. When the strata, according to his system, were elevated from the bottom of the sea, the removal of so much water, if not performed with unaccountable slowness, must have produced torrents, in all directions, of excessive magnitude, and fully adequate to the effects I have thus ascribed to them.

\* THE modifications of the action of heat, occasioned by pressure, which have been taken into account by no geologist but Dr HUTTON, distinguish his theory from all other igneous theories.

stone rocks externally resemble very porous lavas, but when broken are always found to be quite compact internally, and to contain numerous round nodules of calcareous spar. Near the surface, the nodules, being washed out by rain, have left the cavities which have given rise to this deception. The spherical form of the air holes in lavas, and of the nodules of calcareous spar in whins, seems to have been produced by a cause common to both, the mutual repulsion of two fluids intermixed, but not disposed to unite.

It must be owned, that this theory of calcareous spar is as yet hypothetical; but it is supported by strong analogy, and promises to be of service, by leading to decisive experiments and observations. I cannot help believing, that, by a careful examination of the volcanic countries, facts may yet be discovered which will throw light on this subject. In order to promote and direct such researches, I shall beg leave to state some observations which I made in those countries in 1785, before I was attached to any system of geology.

It is generally supposed, that some lavas of *Ætna* contain calcareous spar and zeolite; but this I conceive to be a mistake. It is true, as I have seen, that many rocks of *Ætna* contain these substances in abundance; but in my opinion these rocks are no lavas, but have flowed subterraneously like our whins, and are the same with them in every respect. A particular district of *Ætna*, comprehending the Cyclopiæ Isles, and the country round *La Trezza*, and the Castle of *Jaci*, is decidedly of this description; and vestiges of the same kind occur in other parts of the mountain. In one place fossil coal has been found, and in another we saw marine shells. In the neighbourhood of *Bronte* we observed a high ridge formed of strata of sandstone and limestone, partly overflowed and concealed by recent lavas, but so placed as to render it evident that its continuation formed no inconsiderable part of the mountain. Thus, *Ætna* being composed, partly of the subterranean, and partly of the external productions.

tions of fire, may be expected to afford numberless opportunities of pursuing the comparison between these two classes \*.

A MOST interesting scene for such a comparison occurs likewise on Vesuvius. The history of this volcano is simpler than that of *Ætna*, for it has been evidently formed, with all its appendages, by the continued action of external eruptions, which have raised it, at some remote period, from the bottom of a sea, occupying all the *Campi Phlegræi*, and washing the surrounding Apennines. The whole volcano seems once to have consisted of a single large cone, the greatest part of which has sunk during some violent eruption, probably that which took place in the time of *PLINY*, leaving a fragment of its basis, now called the mountain of *Somma*. This fragment retains its original shape; and on the side fronting the towns of *Somma* and *Otajan*, the external conical surface, along which the ancient lavas had flowed, is still entire. Fronting the centre of the cone, *Somma* breaks off abruptly, and presents a vertical cragg, some hundred feet in height, which is concave inwards. From the gulf, produced by the ruin of the ancient mountain, though not exactly from its centre, have arisen the explosions which, by repeated accumulation, have formed the present cone of Vesuvius. Next the sea, this cone has extended itself so as completely to cover all remains of the ancient one, forming a continued slope from the crater to the foot of the mountain. On the opposite side it meets the base of the craggs of *Somma*, and forms an angle, into which many successive streams of lava have flowed, producing a narrow horizontal valley, in the form of a crescent, called the *Atrio del Cavallo*. From this valley the craggs of *Somma* present a complete view of the internal structure of the ancient mountain, corresponding, in most things, to what might have been supposed.

THE various substances, deposited successively on the external surface of the ancient cone, being cut vertically in this cragg, their

\* *M. DOLOMIEU* has observed this distinction; but supposes that the masses which we conceive to have flowed subterraneously were erupted at the bottom of the sea.

their succession is distinctly seen, the section of each stratum presenting to the view part of a horizontal circle; the whole consists of alternate layers of thin streams of lava, and very thick beds of loose frothy *rapilli*, which last being thrown into the air in a soft state, had fallen in showers on the sides of the mountain.

IN various places the regularity of this arrangement is interrupted by certain vertical lavas, from two feet to ten or twelve in thickness, which cross the strata just described in an irregular manner, and pass upward, without distinction, through the solid beds, and through the loose ones. It immediately occurred to us\*, that these lavas must have flowed in fissures of the ancient mountain; and we accounted for them by supposing, that a melted stream, flowing along the external surface, had met in its course with one of those crevices which are formed in all great eruptions, and had flowed into it so as to return again into the heart of the mountain. This conjecture very nearly agrees with those advanced by M. DOLOMIEU, and by M. BREISLACK, who both mention these vertical lavas of Somma†.

I HAVE since been induced to consider this phenomenon, which formerly seemed to present only an amusing variety in the history of volcanic eruptions, as of the utmost consequence in geology, by supplying an intermediate link between the external and the subterraneous productions of heat. I now think, that, though we judged rightly in believing those lavas

to

\* I saw this place in company with Dr J. HOME in 1785.

† M. DOLOMIEU conceives these lavas to have flowed over the lips of the crater, (*Iles Ponces*, p. 100.); M. BREISLACK, that they had first filled the open cavity of the crater, and from thence had flowed into crevices formed in its sides, "che una lava avendo riempita la cavita del cratere si fosse insinuata per queste fenditure," (*Topografia Fisica della Campania*, p. 115.). This last mentioned work, published in 1798, contains many interesting and accurate descriptions. Should the circumstances of the times permit, the author will have it in his power to follow out, with every advantage, the hints I have suggested.

to have flowed in crevices, we were mistaken as to their direction; for instead of flowing downwards, I am convinced they have flowed upwards, and that the crevices have performed the office of pipes, through which lateral explosions have found a vent. This will appear in the highest degree probable, when we attend to the known history of volcanic eruptions. It generally happens, that the lava begins to flow from the summit, in consequence of the crater being filled with liquid matter up to the brim. At that moment the basis of the mountain must be pressed outwards by a very great hydrostatical force, equal to the weight of a column of liquid lava as high as the mountain itself. It is natural then to expect, that this pressure, assisted by strong percussions of explosion, should lacerate the body of the hill, and form great rents. The lava, urged upwards by the same pressure, would flow through these rents, and emerge at the surface with violence. The discharge would continue through this channel till the propelling force had ceased, when the rents would be left full of lava; which, cooling in that position, would produce vertical lavas, such as those of Somma. This supposition is confirmed by various phenomena: The lava ceases to flow from the crater as soon as a lateral eruption has begun; when it rushes with such violence from the side of the mountain as to fly to a great height into the air, like a *jet d'eau*; and it often makes its appearance, in the same instant, at various mouths, which are not scattered at random, but placed in one continued line, indicating the discharge from a rent. Some circumstances likewise, which I observed on a close examination of the vertical lavas, indicate that the crevices had performed the office of pipes. Frequently the substance at the middle differs from that on each side, whilst the sides resemble each other exactly. I explain this, by supposing that the lava, which had first flowed through the pipe, and had coated its sides with solid matter, had been followed

lowed by a stream somewhat different, which had remained there on the cooling of the whole. In one case, I found lava on each side, and in the middle tuffa, which is generally supposed to have been erupted in the state of watery mud. In another, the substance in immediate contact with the mountain is vitreous, the rest being common lava. This is fully explained by our experiments, if we suppose the stream to have flowed into a cold crevice.

To apply these observations to the general history of the globe: It is evident that the vertical lavas bear the closest resemblance, in point of position, to veins of every description, which, in all parts of the world, are found penetrating the strata, and which, according to Dr HUTTON, have flowed by means of subterraneous heat. The veins, or dikes, (as they are called), of whinstone, which so commonly occur in this country, differ from them in no circumstance which I had the means of observing. It is therefore natural to expect that, if examined with particular care, their agreement will be found complete. Of this, however, we must not form too sanguine expectations; for though the vertical lavas of Somma have undoubtedly sustained the pressure of a great superincumbent mass, we have no proof that this force was sufficiently strong, as Dr HUTTON supposes was the case in whinstone, to repress the volatility of carbonic acid. On the other hand, as we are yet entirely ignorant of the degree of force requisite for this purpose, we have no proof that it has been too weak. All the veins of Somma I examined were absolutely compact, except one which was full of pores. I am unable to determine whether this was the real porosity of a lava, or whether, as in our whins, it arose from the removal, near the surface, of nodules of calcareous spar. Granting that these pores were real air holes, the circumstance was peculiar to that single stream, and may have been owing to an inferior degree of pressure; for, in this respect, we have reason to look for

the greatest diversity. Some vertical streams must have flowed while the mountain was yet low; others may have found vent at a low level; in both which cases, the pressure would be feeble: whereas other streams, communicating with elevated lateral eruptions, would sustain, in their lower parts, the full reaction of deep columns of liquid lava, and may be expected to exhibit the effects of great pressure. Should any future traveller be fortunate enough to meet with a nodule of calcareous spar in a lava, occupying the crevice of a mountain formed by undoubted external eruptions, all that has been said of the effects of pressure would cease to be hypothetical, and this fundamental article of Dr HUTTON's theory would be established beyond dispute.

I HAVE now examined the relation between whinstone and lava in various points of view; and the result of the investigation, by showing the intimate connection between the two classes, tends strongly to confirm the ideas of Dr HUTTON. I flatter myself, likewise, that the experiments, independently of the general views of geology, are of some value, by accounting for the stony character of lavas, and thus enabling us to dispense with the various mystical suppositions which have of late perplexed the history of volcanic phenomena.

TABLE



TABLE OF FUSIBILITIES.

	<i>Substances.</i>	<i>Original softened.</i>	<i>Glass softened.</i>	<i>Cryſtallite softened.</i>
No. 1.	Whin of Bell's Mills Quarry.	40.	15.	32.
2.	Whin of Caſtle Rock.	45.	22.	35.
3.	Whin of Baſaltic Co- lumn, Arthur's Seat.	55.	18.	35.
4.	Whin near Dudding- ſtone Loch.	43.	24.	38.
5.	Whin of Salifbury Craig.	55.	24.	38.
6.	Whin from the Water of Leith.	55.	16.	37.
7.	Whin of Staffa.	38.	14½.	35.
No. 1.	Lava of Catania.	33.	18.	38.
2.	Lava of Sta Venere, Pie- dimonte.	32.	18.	36.
3.	Lava of La Motta.	36.	18.	36.
4.	Lava of Iceland.	35.	15.	43.
5.	Lava of Torre del Gre- co.	40.	18.	28.
6.	Lava of Vefuvius, 1785.	18.	18.	35.

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IV. *A CHEMICAL ANALYSIS of THREE SPECIES of WHINSTONE, and TWO of LAVA. By ROBERT KENNEDY, M. D.*  
F. R. S. & F. A. S. EDIN.

[*Read, December 3. 1798.*]

ON the 5th of August last, I announced to the Society that I had discovered foda in several varieties of the whinstone\* of Scotland, and also in lava from Mount Ætna; but did not describe the various experiments to which these substances had been subjected in my examination of them. In the following paper, therefore, I have the honour of laying an account of these experiments before the Society.

ANALYSIS I.

*Basalt of Staffa.*

THE specimen of this basalt, submitted to the following analysis, was given me by a gentleman, who brought it himself from the celebrated basaltic columns in Staffa. A description of its external mineralogical characters may be found in Sir JAMES HALL's paper, (p. 55. of this volume), to which I beg leave to refer.

THIS

\* THE name whinstone is used throughout this paper in a generic sense, comprehending basalt, trap, certain kinds of porphyry, wacken, and some other stones of the argillaceous class.

THIS basalt, though reduced to fine powder, does not effervesce with acids. The colour of the powder is greyish, and when wet greenish. By being exposed to a low red heat, the colour of the stone is changed to brown. It is not attracted by the magnet, either in its natural state, or after being heated red hot.

ITS specific gravity, taken in distilled water at the temperature of  $60^{\circ}$  of FAHRENHEIT, I found to be 2.872.

SOME small pieces being exposed to a low red heat for half an hour, lost 5 *per cent.* in weight; and when the stone was reduced to powder, and heated red hot, the loss was the same. I also examined the effects of high heat on it, in the following manner. Having made some small crucibles of the porcelain clay of Cornwall, which I used on account of its great purity and infusibility, I baked them in pretty strong fires, generally above 100 of WEDGWOOD. As soon as they were cold, they were each exactly weighed. A portion of the basalt in fragments, also weighed, being put into one of these small crucibles, and a pyrometer into another of the same size, both were placed in a Hessian crucible. A small flat cover, also made of the porcelain clay, was laid upon each; and then a lid was carefully luted on the Hessian crucible with clay and sand. The apparatus thus prepared was next set into a furnace; and the fire being raised gradually till it appeared to have attained the pitch desired, it was kept as equal as possible for about an hour. The small crucible, and the melted basalt it contained, being weighed as soon as cold, it was easy to determine how much weight was lost.

IN this manner some of the basalt was exposed to a heat of  $72^{\circ}$  of WEDGWOOD, at which it was vitrified, and lost exactly the same weight as in a low red heat. At  $160^{\circ}$  the effects were in every respect the same; the loss not being greater in that intense fire. The small crucibles, in which the pyrometers had been

been placed, did not in these experiments lose the smallest weight.

THE volatile matter thus driven off by heat is partly water, as the following experiment shows: I put half a pound Troy of the basalt in fragments, into a small WEDGWOOD retort, and luted to it a receiver, into an aperture of which was fitted one end of a glass tube, the other end being adapted to a pneumatic apparatus. The retort was then heated slowly to redness, and kept moderately red hot for two hours. In the receiver some water was condensed. Some gas also passed over; but I could not ascertain with precision either its quantity or its nature, as it was mixed with the air of the receiver. I have not made farther experiments on the volatile matter contained in whins; but it deserves to be examined with attention.

THIS basalt being exposed to heat in a muffle, was found to soften at 38 of WEDGWOOD\*.

SOME of it being reduced to fine powder, was boiled in thirty times its weight of water for half an hour. After filtration the water was examined with different chemical tests, but gave no precipitate with any, except a slight cloud with nitrate of silver; and a portion being evaporated to dryness, left only some thin streaks on the bottom of the glass.

HAVING premised these particulars, I proceed to describe the analysis.

I. ONE hundred grains of the basalt, reduced to fine powder in a WEDGWOOD mortar†, were mixed, in a small retort, with  
about

\* THE fusibility of this, and the other substances to be afterwards mentioned, I examined with Sir JAMES HALL. For this purpose, a small piece of each was placed, with a pyrometer as near to it as possible, in an open muffle previously heated to redness. It could thus be seen perfectly during the operation, and the fire being raised, as soon as it was found to be soft, when pressed slightly by an iron rod, the degree of heat was ascertained by measuring the pyrometer.

† THE mortar I used was not scratched by any of the whins or lavas mentioned in this paper.

about 1200 grains of muriatic acid; and a receiver being adapted, the mixture was gradually heated till it boiled. It was at first of a brownish-yellow colour, but afterwards became brownish. Part of the powder was dissolved. To distill off the uncombined acid, the heat was continued till the mixture began to grow thick. It was then diluted with boiling distilled water, and poured on a filter; and the undissolved part, after properedulcoration, being dried and heated red hot a few minutes, weighed  $67\frac{1}{2}$  grains, and was greyish-white.

2. THE filtered solution was of a faint yellowish-brown colour. Being saturated with caustic ammonia, a bulky precipitate was thrown down, which was carefully separated by filtration. It had at first a dirty greenish-colour, which was afterwards changed to brown by the action of the air.

3. THE solution, after being freed from this precipitate, was perfectly colourless and transparent. I dropt into it a small quantity of sulphuric acid, which produced no cloud; consequently the solution contained neither barytes nor strontian. It was then evaporated to a small quantity, and treated with carbonate of ammonia. Some white earth was thrown down, apparently carbonate of lime, which after being washed, dried, and heated red hot a few minutes, weighed  $6\frac{1}{2}$  grains.

4. THE insoluble residuum, No. 1. which weighed  $67\frac{1}{2}$  grains, I mixed in a silver crucible with a solution of caustic potash, containing as much alkali as was equal to twice the weight of the residuum. This mixture, being evaporated to dryness, was exposed for one hour to a red heat, in which it melted. When cold, the mass was green. After being softened, and washed out of the crucible with boiling distilled water, it was superaturated with muriatic acid, by which the greater part was dissolved. This mixture, being then evaporated to a small quantity, became gelatinous. It was next diluted with water, digested, and filtered. Some filix remained on the filter, which, after proper

proper washing, being dried, and heated red hot a quarter of an hour, weighed 43 grains, and was perfectly white. To learn whether this filix was free from every other earth, I mixed a part of it with four parts of carbonate of soda, and melted the mixture in a silver crucible. Water, being poured on the melted mass, dissolved it entirely into a liquor filicum, which was diluted largely, and saturated exactly with an acid. No precipitate appeared, even after six or eight days; therefore these 43 grains were pure filix.

5. THE solution, No. 4. (from which the filix had been separated), was of a light greenish colour. Caustic ammonia, when poured into it, threw down a brownish precipitate. Having carefully separated this precipitate, and washed it on a filter, I dropt into the remaining solution, which was now colourless and transparent, a small quantity of sulphuric acid, in order to detect barytes or strontian. No precipitate was formed. The solution was then evaporated to a small quantity, and treated with carbonate of ammonia, by which a second portion of carbonate of lime was obtained. Its weight, after being heated red hot, was  $9\frac{1}{2}$  grains.

6. THE brownish precipitates, thrown down from the solutions No. 2. and 5. by caustic ammonia, had the appearance of argil mixed with iron. To separate the argil, these precipitates were mixed together, and boiled, while still moist, in a solution of caustic potash, in a silver crucible. A part was dissolved; but a spongy matter remained, of a darker brown colour than at first, which was collected on a filter.

7. INTO the caustic-alkaline solution I poured sulphuric acid, till slightly in excess, and neutralised it again by carbonate of soda. The argil was precipitated; which being sufficiently washed, was redissolved in diluted sulphuric acid. This solution was then mixed with some acetite of potash, and gave, by successive evaporations, small regular crystals of alum. At last it

it became gelatinous; and being evaporated to dryness, and diluted again with water, 1 grain of filix was left. The remaining solution produced, to the last drop, crystals of alum\*. I dissolved these crystals in water, and precipitated the argil by carbonate of ammonia. After being carefully washed, dried, and heated red hot a quarter of an hour, it weighed 12 grains.

8. THE brownish matter, No. 6. insoluble in caustic potash, seemed to be oxyd of iron; and after having been heated red hot, weighed  $24\frac{1}{4}$  grains. I powdered this mass, and poured on it some acetous acid, in order to detect magnesia; but nothing was dissolved. It was next treated with nitric acid, which dissolved the iron, but left 4 grains of filix. The iron being precipitated, dried, and heated red hot, weighed 20 grains, and was magnetic. Suspecting that some argil might still be mixed with it, from having escaped the action of the caustic potash, I dissolved 5 grains in muriatic acid, and precipitated the iron by Prussian alkali. Having separated the blue precipitate, I boiled the solution with carbonate of soda, and obtained 1 grain of argil. These 20 grains consisted, therefore, of 16 grains of iron, and 4 grains of argil.

THE remaining part of the iron was melted for an hour, with ten times its weight of nitre, in order to detect manganese. The mixture, however, when cold, was not greenish; and water made a colourless solution of the saline matter, which did not become turbid, when exposed some days to the action of the air †.

PART I.

I.

9. THE

\* It therefore contained none of the earth which VAUQUELIN lately discovered, and to which he has given the name of glucine.

† I THINK, however, it is probable, that this basalt contained a small quantity of manganese, both from the brownish colour of the solution, No. 1. and from the green colour which the undissolved residuum gave, by fusion; with caustic potash.

9. THE two portions of white earth above mentioned, which seemed to be carbonate of lime, weighed together 16 grains. To separate magnesia, if any were mixed with this earth, I put it into a little water, and added fulphuric acid till slightly in excess. Sulphate of lime was produced. Having poured some alcohol into this mixture, I filtered it, and washed the sulphate of lime with more alcohol diluted with water. The filtered liquor was then boiled with carbonate of soda, but no magnesia was precipitated.

THESE 16 grains were therefore carbonate of lime, of which, according to Mr KLAPROTH's calculation, about 9 grains were pure lime.

ONE hundred parts of the basalt of Staffa contain, according to the above analysis :

Silex,	-	(No. 4, 7. and 8.).	-	48
Argil,	-	(No. 7. and 8.).	-	16
Oxyd of iron,	-	(No. 8.).	-	16
Lime,	-	(No. 9.).	-	9
Moisture, and other vol. matter,	-	-	-	5
				<hr/>
				94

THE sum is 94 parts ; consequently there is a loss of 6 *per cent.*

ABOUT a year ago I analyzed specimens of some of the whins in the neighbourhood of Edinburgh, and found, that the sum of the earths and iron, separated by the analyses, never amounted to more than 93 or 94 *per cent.* ; so that the loss was always equal to that just mentioned. It was this circumstance which first led me to suspect that some saline substance existed in



in these stones; and their considerable fusibility favoured the suspicion.

SOON after these analyses were made, I observed another circumstance, which amounted to an absolute proof of the whins containing something of a saline nature, in combination with their earthy bases. Most of the artificial crystallites, made by Sir JAMES HALL, which I had always an opportunity of examining, threw out on their surfaces, two or three weeks after their formation, a white efflorescence, which had a very salt taste. It was in too small quantity to be collected and examined; but when washed off, it was often formed a second time.

I WAS thus convinced of the existence of some saline substance in these bodies, and made different experiments with several of them, in order to separate it, and ascertain its nature; and soon found that it was soda.

I SHALL next describe some of the methods by which this alkali was most easily separated from the earthy parts of the whins.

*Experiments to obtain the Soda, and determine its Quantity.*

HAVING broken some of the basalt of Staffa to small fragments, I weighed 400 grains, and ground the whole with water to an extremely fine powder, in a WEDGWOOD mortar. The powder, and the water with which it had been ground, were then put into a small retort, and mixed with about 1200 grains of sulphuric acid, which I had carefully distilled for this operation. I placed the retort in the sand bath of a small furnace which I use for analyses, adapted a receiver, raised the fire till the acid began to distill slowly, and carried on the distillation to dryness. Water was then poured into the retort, and boiled, the mixture thrown on a filter, and the undissolved residuum sufficiently washed. This residuum was next treated a second time

time with a fresh portion of sulphuric acid; and afterwards boiled with water, filtered and washed, exactly in the same manner as before. The undissolved part of the stone was now almost white.

THE filtered solutions being mixed together, were evaporated to dryness; and the saline mass which remained was heated red hot for one hour in a clean and new Hessian crucible. When cold the mass was of a brick red colour. Having powdered it well, I boiled it in water, poured the whole on a filter, and washed the reddish matter carefully. This filtered liquor, in which all the soda, separated from the basalt by the sulphuric acid, was dissolved in the state of sulphate of soda, could contain only a small quantity of earthy matter; for the greater part of the sulphate of argil, and of iron, formed by the first part of the process, must have been decomposed by the red heat, to which the mass was afterwards exposed. Accordingly, the solution being treated with carbonate of ammonia, only a small quantity of a precipitate was thrown down, which was carefully separated. The solution was then evaporated to dryness. A saline mass remained, consisting, in part, of sulphate of ammonia; to separate which the whole was exposed to heat in a small crucible, and when it ceased to emit fumes, the heat was increased to redness. A fixed white salt was left, which weighed 25 grains.

THIS salt I redissolved in water, added some carbonate of ammonia, and heated the mixture till it boiled. A small quantity of an earthy precipitate was again thrown down, which being separated by filtration, the solution was evaporated to dryness, and the salt which remained heated red hot a second time. By these successive operations, all the earthy matter, at first dissolved, was separated. The salt now weighed 23 grains, and had all the properties of sulphate of soda. These properties were the following:

1. IT was not volatile in a moderate red heat.  
 2. AFTER being thus dried, it dissolved readily in about six times its weight of water, at the temperature of 60 of FAHRENHEIT.  
 3. THIS solution gave, by evaporation, crystals exactly the same in form as artificial sulphate of soda; and these crystals effloresced in dry air.

4. A PART of the solution of this salt being boiled with carbonate of soda, gave no precipitate; a proof that it contained no earthy matter.

5. SOME of the solution being mixed with a very strong solution of acid of tartar, remained unaffected; the salt therefore contained no potash.

6. SOME of the salt being dissolved in water, was decomposed by nitrate of barytes; and the sulphate of barytes produced was separated by filtration. The nitric acid, thus united to the alkaline basis, formed a saline compound, which, in the next place, was mixed and deflagrated with charcoal. By washing the coaly residuum, and evaporating the water, I obtained pure carbonate of soda, which effloresced readily in the air.

THERE can be no suspicion of the retort which was used furnishing any part of the alkali; for I weighed it previously in a balance of great accuracy; and after the operation was finished, found its weight exactly the same as at first, and the lustre of the glass altogether unimpaired.

THE whin which I next submitted to examination, for the purpose of separating the soda, was taken from a quarry near the Water of Leith\*. I used a considerable quantity, 800 grains; which were distilled twice with sulphuric acid, and then treated in every respect exactly as the preceding. The sulphate of soda obtained, amounted to 43 grains.

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\* THIS species is the first mentioned in Sir JAMES HALL's paper. When powdered, it effervesced slightly with acids. I did not analyze it; but, in the course of the process for detecting soda, one of the earthy precipitates proved, upon examination, to be magnesia. It is the only whin in which I have found this earth.

I AFTERWARDS subjected some other whinstones to the same kind of processes, and in each species found soda. The nitric and muriatic acids also dissolve a certain quantity of the alkali contained in these substances; but their action is weaker than that of the fulphuric acid.

By the experiments now described, there were separated from 100 parts of each of the whins, between five and six parts of sulphate of soda, which may be equal to two or three parts of pure soda. But as these two or three parts, when added to the sum of the earths and iron, did not account for the loss of 6 or 7 *per cent.* always observed in my analyses, I was satisfied that the whole of the alkali was not obtained by the processes which were followed; even although, in that with the whin from the Water of Leith, it had been exposed, in very fine powder, to the action of the fulphuric acid, at a boiling heat, for more than eighteen hours. It appeared necessary, therefore, to try other methods; and after some consideration it occurred to me, that if the powdered whins could be exposed, while red hot, to the vapours of the fulphuric acid, also in a red hot state, its power in separating the whole of the alkali from the earthy bases of these substances, would probably be greatly increased in so high a temperature. I succeeded in applying a red heat both to the powdered stone, and to the acid at the same time, by the following means.

SOME of the basalt of Staffa being mixed, in very fine powder, with three parts of fulphuric acid, the mixture was evaporated slowly to dryness in a sand bath. The dry mass was then heated gradually to redness, and kept in the fire for one hour. It was next powdered, and boiled in water; and the water being filtered, was treated with carbonate of ammonia, which threw down a small quantity of a brownish precipitate. After separating this precipitate by filtration, the liquor was evaporated to dryness, and the sulphate of soda, which was left, was purified

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in the manner already described, and heated red hot. It amounted to 9 parts for every 100 parts of the basalt employed.

IN this experiment, the fulphuric acid was first united to a part of the lime, of the argil, and of the iron, contained in the stone; and afterwards, when the mass was exposed to a red heat, the acid was driven off partly or wholly from these, and applied in red hot vapours to every part of the powder; by which its action appears to have been rendered much more powerful, as 9 *per cent.* of sulphate of soda was produced: and by the same process, so simple and easy to execute, I got from the rest of the substances, to be mentioned in this paper, from 8 to 11 *per cent.* of sulphate of soda, although, when they were merely boiled in the acid, the quantity of this salt never exceeded 5 or 6 *per cent.*

As the proportion of acid and alkali in neutral salts has not been hitherto determined with certainty, the quantity of soda in these whins cannot be exactly known. But it is probable that 9 parts of sulphate of soda, dried by a red heat, do not contain less than  $3\frac{1}{2}$  or 4 parts of pure alkali\*; which must therefore be considered as the weight in 100 parts of the basalt of Staffa: and as  $3\frac{1}{2}$  or 4 parts of soda, when added to the sum of the earths and iron, amount nearly to the 100 parts of the stone employed in the analysis, this calculation may be reckoned very near the truth. For the same reasons I think it likely, that the greater part, or the whole of the soda, was obtained from the basalt by the process which has been last described.

It is well known among the friends of the late Dr HUTTON, that he made some experiments on zeolite; by which he concluded, that soda entered into the composition of that substance.

\* This is nearly the proportion given by Mr KIRWAN.

stance\*. He has not mentioned the circumstance in any of his works; but Dr BLACK has been accustomed, as he informed me himself, to take notice of it in his lectures on chemistry, for many years. It is my intention to analyze some species of zeolite; and if the results seem of any importance, they shall be laid before the Society.

AMONG the experiments on the basalt of Staffa, already described, it has been observed, that, when the powder was boiled in water, a slight precipitate was produced in the water by nitrate of silver, thus indicating some traces of muriatic acid. As it appeared of importance to determine how much of this acid the basalt contained, I subjected some of it to examination for that purpose.

*Experiments to ascertain the Quantity of Muriatic Acid in the Basalt of Staffa.*

ONE hundred grains of the stone, in fine powder, were mixed in a small retort with some nitric acid; and a receiver being adapted, the mixture was boiled gently, till the greater part of the acid had distilled over. The liquor in the receiver being examined with nitrate of barytes, remained unaffected; but gave a slight cloud with nitrate of silver, which shewed that it contained some muriatic acid.

THE mass in the retort being diluted with water, the whole was filtered; and this filtered liquor produced no cloud with nitrate of barytes, but gave, like the former, a slight precipitate with nitrate of silver.

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\* IN the 26th volume of the *Annales de Chimie*, p. 119. M. SCHERER, in a letter to VAN MONS, says, that he was informed by Dr BLACK, that Dr HUTTON had, long ago, found *potash* in zeolite. In this statement M. SCHERER is incorrect; because it was soda, as above mentioned, which Dr HUTTON obtained from that substance.

IN the next place, the undissolved residuum was mixed with twice its weight of very pure caustic potash, and exposed to a low red heat, for an hour, in a silver crucible. The mass was then diluted with water, supersaturated slightly with nitric acid, and filtered. With this solution nitrate of barytes produced no effect; consequently, these experiments show, that the stone in question does not contain any traces of sulphuric acid. With nitrate of silver, however, the solution gave a white precipitate, more abundant than the two preceding. The different portions of muriate of silver, being collected, and dried on a sand bath, weighed only 4 grains.

As a fourth part of muriate of silver consists of acid, according to the most correct experiments hitherto published, these 4 grains consequently indicate, in 100 parts of this basalt, only about one of muriatic acid. All the whins and lavas, to be mentioned in the remaining part of this paper, were found, by a similar process, to contain about the same quantity.

ACCORDING to the results of these different processes, there are, in 100 parts of the basalt of Staffa,

Silex,	-	-	-	48
Argil,	-	-	-	16
Oxyd of iron,	-	-	-	16
Lime,	-	-	-	9
Moisture, and other vol. matter,	-	-	-	5
Soda, about	-	-	-	4
Muriatic acid, about	-	-	-	1
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				99

I HAVE thus detailed exactly the various experiments performed in analyzing this species. As all the other whins, and the lavas, which follow, were analyzed in the same manner, and

PART I.

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exhibited

exhibited nearly the same chemical properties, the results only, in each example, shall be mentioned.

## ANALYSIS II.

*Whin of Salisbury Rock.*

THE specimen employed was chosen from the fourth side of the cragg, and was perfectly hard and free from decomposition, as the particular spot from which I broke it had been quarried a short time before.

A DESCRIPTION of its external characters may be found in p. 54. of this volume. Its powder is light greenish-grey; but after being wet, acquires a dirty green colour. When heated to redness, it becomes light brown. Though not attracted by the magnet in its natural state, it becomes magnetic after being heated red hot. It does not effervesce with acids. When exposed to a low red heat for half an hour, whether in fragments or in powder, it loses 4 *per cent.* in weight. It softens at 55 of WEDGWOOD. Its specific gravity is 2.802. After being boiled, in the state of fine powder, in ten or twelve parts of muriatic acid, the insoluble residuum amounted to 65 *per cent.*

I ANALYZED 200 grains of this whin, and found that it contained, in 100 parts,

Silex,	-	-	-	-	46
Argil,	-	-	-	-	19
Oxyd of iron,	-	-	-	-	17
Lime,	-	-	-	-	8
Moisture, and other vol. matter,	-	-	-	-	4
Soda, about	-	-	-	-	3.5
Muriatic acid, about	-	-	-	-	1
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					98.5

ANALYSIS



ANALYSIS III.

*Whin of the Calton Hill, near Edinburgh.*

THE rock of this hill varies much in different parts ; but its general character is that of porphyry. The piece I chose for analysis was taken from the south side, about ten or twelve feet below its highest point ; and was free from calcareous spar.

THE external characters of this particular piece are as follow : It consists of a greyish basis, containing rhomboidal crystals of felspar of a light reddish-brown colour ; and small spherical masses of green earth. The basis, in its fracture, is uneven, and earthy, and has no lustre. It can be scratched easily with a knife, and gives an earthy smell when breathed on. The green earth is soft, and is affected in some degree by water ; and being decomposed by the weather, as well as the veins and nodules of calcareous spar, which are very common in this hill, the rock in many places is extremely porous.

THE specimen I have described may be called argillaceous porphyry. It effervesces slightly with acids ; so that the lime which it contains must be united to carbonic acid. Its powder is light grey, with a certain shade of purple. When heated to redness, it becomes of a brown colour. It is not attracted by the magnet, either in its natural state, or after ignition. By being exposed to a low red heat for half an hour, it loses 5 per cent. of its weight. It softens at 44 of WEDGWOOD. Its specific gravity is 2.663, as nearly as I could ascertain, from the effect which the water had in making it crumble down.

ONE hundred parts contain,

Silex,	-	-	-	-	50
Argil,	-	-	-	-	18.50
Oxyd of iron,	-	-	-	-	16.75
Carbonate of lime,	-	-	-	-	3
Moisture, and other vol. matter,	-	-	-	-	5
Soda, about	-	-	-	-	4
Muriatic acid, about	-	-	-	-	1
					<hr/>
					98.25

#### ANALYSIS IV.

##### *Lava of Catania. Ætna.*

THIS lava, and the species to be next mentioned, were brought from Mount Ætna by Sir JAMES HALL and Dr JAMES HOME. At their request I analyzed specimens of each. Mineralogists are well acquainted with these lavas, from the descriptions which have been given of them by M. DOLOMIEU ; therefore it is unnecessary for me to mention their external characters.

THE lava of Catania gives a powder of a light grey colour, which is very little changed in appearance by being heated red hot. After it is wet, it becomes dark grey. When this lava is reduced to small fragments, some parts of it are attracted by the magnet, and others are not. In fine powder it is but feebly attracted ; and after ignition its qualities in this respect do not seem to be altered. It softens at 33 of WEDGWOOD. The specific gravity of the pieces most free from air bubbles, is 2.795.

I HAVE exposed this lava, in the manner already described, to various degrees of heat, from redness to 158 of WEDGWOOD, and constantly observed that it never lost the smallest weight.

WHEN

WHEN boiled like the whins in muriatic acid, the part remaining undissolved weighed 68 *per cent*.

THERE are in 100 parts,

Silex,	-	-	-	-	51
Argil,	-	-	-	-	19
Oxyd of iron,	-	-	-	-	14.50
Lime,	-	-	-	-	9.50
Soda, about	-	-	-	-	4
Muriatic acid, about	-	-	-	-	1
					<hr/>
					99.

#### ANALYSIS V.

*Lava Sta Venere, Piedimonte, Ætna\*.*

THIS species gives, like the preceding, a greyish powder, which becomes dark grey when wet, and its colour is scarcely affected by being exposed to a low red heat. It is but feebly attracted by the magnet, whether in small fragments or in powder; and its qualities in this respect are not changed by low ignition.

IT softens at 32 of WEDGWOOD, and does not lose any weight in fires between 150 and 160. After being boiled in muriatic acid, it left 68 parts *per cent*. undissolved. Its specific gravity is 2.823.

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\* DOLOMIEU, in describing this lava, says, that its fracture is conchoidal, like that of flint. The specimen which I analyzed had an uneven fracture; and its colour was blackish-blue: in other respects, however, it answered to DOLOMIEU's description.

IN 100 parts I found,

Silex,	-	-	-	-	50.75
Argil,	-	-	-	-	17.50
Oxyd of iron,	-	-	-	-	14.25
Lime,	-	-	-	-	10
Soda, about	-	-	-	-	4
Muriatic acid, about	-	-	-	-	1

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97.50

IN analyzing these two lavas, I examined the different solutions, with particular attention, for magnesia, and sulphuric acid; but could not detect any traces of either of these substances.

THE results of these analyses show, that whins, and a certain class of lavas, taken from remote quarters of the globe, consist of the same component elements, united in each, nearly in the same proportion. The only circumstance in which they materially differ, is the loss of some volatile matter in the fire, which is peculiar to the whins alone.

WE need not be now surpris'd at the facts mentioned by DOLOMIEU, and others, of soda being found about volcanos, or upon the surface of lavas; as it has thus been shown to exist in these substances in combination with their earthy bases.

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THE facts and experiments I am next to mention will prove, that whins and lavas are not the only stones which contain soda;

da ; and will even render it probable, that this alkali is widely diffused through mineral bodies.

Soon after I first discovered it in whins, and had communicated the circumstance to Sir JAMES HALL, he sent me, from a high sandstone rock in his estate, a quantity of the stone decomposed ; and informed me that there was a saline efflorescence mixed with it, which was collected along with the loose matter, and which seemed, by the taste, to be sea-salt. The place has been long called by the common people the *Salt Heugh*. By simply boiling some of the decomposed sandy part in water, and afterwards filtering and evaporating the water, I obtained regular crystals of sea-salt, mixed with a small quantity of sulphate of soda.

HENCE it appeared likely, that common salt would be found to be one of the component parts of ordinary sandstone strata. To verify this important observation, I next examined two hard and solid specimens, taken from some depth below the surface, and perfectly free from decomposition. The first was broken from a quarry about two miles to the westward of Edinburgh. A portion of it being reduced to minute grains, of such a size as the particles of the stone seemed to have consisted of originally, was mixed with some diluted nitric acid, and boiled with it gently for three hours. The acid, after being filtered, was examined with nitrate of barytes, with which it produced only a slight cloud. But nitrate of silver, when poured into it, threw down a copious white precipitate of muriate of silver, indicating the presence of muriatic acid.

AFTER this precipitate was separated by filtration, the liquor which passed through was evaporated to dryness. A saline matter remained, which being mixed with some charcoal, and heated, deflagrated like nitrous salts. Having washed the coaly residuum, and evaporated the water, I got some perfectly pure carbonate of soda. There had been, therefore, in the sandstone,

stone, some common salt, which, by this process, was decomposed, and its acid and alkali collected separately; but whether the whole of the salt was obtained, and what proportion it bore to the earthy parts, I cannot determine, as the stone itself was not analyzed.

THE next specimen was taken from a stratum of sandstone, which lies below the hill of Salisbury Craig; and I chose this species, because the whin to which it is contiguous has already been shown to contain soda. Some of it being reduced to the state of fine sand, and treated in every respect as the preceding, gave a portion of muriate of silver, and of carbonate of soda. The existence, therefore, of sea-salt in these varieties of sandstone, is thus fully established\*.

THE celebrated Mr KLAPROTH of Berlin has already shown, that potash enters into the composition of several stony substances; and by the experiments described in this paper, the other fixed alkali, soda, has also been proved to exist in mineral bodies, as it has been separated from nine different varieties; all of which also contain a certain quantity of muriatic acid.

As caustic fixed alkali was much used in these analyses, I shall conclude this paper by describing, in a few words, the method by which I prepare it; both because its purity is of the greatest importance, and because the process I employ differs, in some circumstances, from that of most chemists. Having obtained an alkali free from all earthy matter, either by burning white tartar, or by repeated solutions and crystallizations of carbonate of soda, I dissolve it in a considerable quantity of water. The requisite proportion of lime being slacked, and allowed to cool, it is diluted with water, and then mixed with the solution  
of

\* SINCE these experiments were performed, I have seen several decomposed sandstones, on the surfaces of which there was an efflorescence of common salt.

of the alkali. This mixture is frequently stirred during two or three days; for when no heat is applied, the lime requires a certain time to attract the whole of the carbonic acid. In the next place, the mixture is filtered through a piece of linen placed in a funnel, and water poured on it till the whole of the alkali is washed out. As it passes through, it is poured, at intervals, from the bottle which receives it first, into a second that is closely stopped.

IN making the fixed alkalis caustic, it is usual to boil them with the lime; but as most kinds of limestone contain a small quantity of flint or argil, and as these earths, when in a state of division, are soluble in boiling caustic alkali, there is a probability, when heat is applied, of its being thus rendered impure. This is my reason for carefully avoiding heat in the first part of the process; and I have not found that lime made from chalk, or from the purer kinds of limestone \*, give any impurity whatever to the alkali, when mixed with it cold.

THE solution, in its weak state, is first evaporated in a basin of hammered iron, polished; but when it is somewhat concentrated, I carry on the evaporation in a dish made of the purest silver, reduced from luna cornea. After being boiled to a small quantity, it is allowed to cool, and then put into a well-stopped bottle for some days; during which, if the evaporation has been continued long enough, the neutral salts crystallize, as Mr LOWITZ has pointed out. Afterwards, the solution is carefully decanted from these salts, and again evaporated in the silver dish, till it acquires the consistence of thin oil †. In this state, so little water is present, that any part of the alkali, which

PART I. N. may

\* Mr KLAPROTH uses lime made of Carrara marble, which he boils with the alkali. (*Beiträge*, vol. i. preface).

† Mr LOWITZ, in describing his process for crystallizing caustic potash, directs the last evaporation to be performed in a glass retort. This method is very erroneous; as the alkali, when heated and concentrated, will dissolve large quantities of the glass.

may be united to carbonic acid, crystallizes, and also any neutral salt that may remain; and the solution being decanted a second time, is obtained perfectly pure, colourless, and transparent.

IN the last boiling it is somewhat difficult to observe the exact degree of concentration at which all the alkaline carbonate will crystallize; and if the evaporation is carried too far, the caustic alkali, if potash, crystallizes itself: so that several evaporations are sometimes requisite.

WHEN no more water remains in the solution than is just sufficient to hold the caustic alkali dissolved, it contains nearly half its weight of alkali; but the exact quantity is easily known, by evaporating a portion to dryness in a silver crucible. Before using such a solution for analyses, I ascertain its purity in the following manner: Some of it being supersaturated with perfectly pure nitric acid, is examined with nitrate of barytes and of silver; with neither of which, if properly made, it will give the smallest cloud; consequently it can contain no sulphuric or muriatic acid.

ANOTHER portion being saturated exactly with a pure acid, the whole is evaporated to dryness, and the salt left is redissolved in a little water. If any earth were contained in the caustic alkali, it would remain thus undissolved; but when made as above described, I have never, in this examination, observed the smallest sediment.

AFTER the alkali is purified from neutral salts, and from the part united to carbonic acid, it may itself be crystallized by farther evaporation, as Mr LOWITZ has shown. But this process seems of no use in chemical analysis, as the alkali is previously obtained altogether pure.



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V. *A NEW METHOD of resolving CUBIC EQUATIONS. By*  
*JAMES IVORY, Esq. Communicated by JOHN PLAYFAIR,*  
*F.R.S. EDIN. and Professor of Mathematics in the University*  
*of Edinburgh.*

[*Read, 6th May 1799.*]

1. **I** DIVIDE cubic equations into two varieties or species : the one, comprehending all cubic equations with three real roots ; the other, all those with only one real root.

2. LET  $\phi$  denote any angle whatever, and let  $\tau = \tan \phi$ , the radius being unity : let also  $z = \tan \frac{\phi}{3}$  : then from the doctrine of angular sections we have

$$\tau = \frac{3z - z^3}{1 - 3z^2},$$

which being reduced to the form of an equation, is

$$z^3 - 3\tau z^2 - 3z + \tau = 0.$$

Now, from what is commonly taught in angular sections,  $z$ , in this equation, may denote, not only  $\tan \frac{\phi}{3}$ , but also  $\tan(\frac{\phi}{3} + 120^\circ)$ , or  $\tan(\frac{\phi}{3} + 240^\circ)$ . It is to be remarked, too, that any value whatsoever may be assigned to  $\tau$ , positive or negative, and without limit or restriction as to magnitude. The equation, then, has three different values of  $z$  for every given value of  $\tau$  ; and it belongs to the species of cubic equations, having three real roots.

N 2

3. AGAIN

3. AGAIN I assume this expression,

$$\frac{1-\tau}{1+\tau} = \frac{(1-z)^3}{(1+z)^3}.$$

LET there be conceived an equilateral hyperbola, of which the femiaxes are each equal to unity, and let a straight line be drawn to touch the hyperbola at its vertex: Conceive also a straight line to be drawn from the centre, to cut off a sector from the hyperbola itself, or from its opposite, or conjugate hyperbolas, and to intercept a part  $\tau$  (estimated from the vertex) on the tangent line: And, in like manner, let another straight line be drawn from the centre to cut off another sector, that shall be one third part of the former sector, and to intercept a part  $z$  on the tangent line: Then the relation of  $\tau$  and  $z$  will be as in the expression here assumed, viz.

$$\frac{1-\tau}{1+\tau} = \frac{(1-z)^3}{(1+z)^3}.$$

I SHALL not stop to demonstrate this proposition respecting the hyperbola: it easily follows from the known properties of that curve. I mention it merely with the view of marking the strict analogy that subsists between the two varieties of cubic equations. It is sufficient for our purpose to remark, what is indeed very evident from the nature of the assumed expression, that, whatever value be assigned to  $\tau$ ,  $z$  has always one real correspondent value, and only one.

FROM our assumed expression we get

$$\tau = \frac{(1+z)^3 - (1-z)^3}{(1+z)^3 + (1-z)^3} = \frac{3z + z^3}{1 + 3z^2},$$

which being reduced to the form of an equation, is

$$z^3 - 3\tau z^2 + 3z - \tau = 0.$$

THIS equation has only one value of  $z$  for every given value of  $\tau$ ; and it belongs to the species of cubic equations having only one real root.

4. IN

4. IN order to give to the two equations, investigated above, the utmost generality of which they are capable, I write  $\frac{\tau}{R}$  for  $\tau$ , and they finally become,

$$\text{I. } Rz^3 - 3\tau z^2 + 3Rz + \tau = 0,$$

$$\text{II. } Rz^3 - 3\tau z^2 + 3Rz - \tau = 0,$$

in which two equations,  $R$  and  $\tau$  represent any numbers, positive or negative, and altogether unlimited and arbitrary as to magnitude.

I CONSIDER the two preceding equations as the simple cases, or simple forms, of the two species of cubic equations: And the method of resolution I have to propose is, to reduce every cubic equation whatsoever to one or other of these two forms.

THE first of the above forms is an equation belonging to the circle. It expresses the relation between the tangent of an arch, and the tangent of the third part of that arch: and it has, in all cases, three real roots. If we take the angle  $\phi$ , of which the tangent is  $\frac{\tau}{R}$ , the radius being unity; the three roots of the equation, or the three values of  $z$ , are,  $\tan \frac{\phi}{3}$ ,  $\tan \left( \frac{\phi}{3} + 120^\circ \right)$ , and  $\tan \left( \frac{\phi}{3} + 240^\circ \right)$ .

THE second of these forms is an equation belonging to the hyperbola. It expresses the relation between the tangents of two hyperbolic sectors, of which the one is triple of the other; and it has, in all cases, only one real root. From the expression assumed (Art. 3.) above, whence this equation was deduced, we get,  $\frac{1-z}{1+z} = \sqrt[3]{\frac{1-\tau}{1+\tau}}$ : therefore,

$$z = \frac{1 - \sqrt[3]{\frac{1-\tau}{1+\tau}}}{1 + \sqrt[3]{\frac{1-\tau}{1+\tau}}}: \text{ or, writing } \frac{\tau}{R} \text{ for } \tau, z = \frac{1 - \sqrt[3]{\frac{R-\tau}{R+\tau}}}{1 + \sqrt[3]{\frac{R-\tau}{R+\tau}}}: \text{ And}$$

so  $z$  is found by extracting the cubic root of a given number.

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THE two forms differ from one another only in the signs of their terms. The first and third terms, as well as the second and fourth, have always unlike signs in the first form; but always like signs in the second form. This property respecting the signs of the alternate terms, by which the one equation is essentially distinguished from the other, I shall denominate the "*Characteristic of the form.*"

5. I PROCEED, now, to shew, in what way any proposed cubic equation may be reduced to one or other of the two forms.

LET the proposed equation be,

$$x^3 + Ax^2 + Bx + C = 0,$$

where A, B, C denote any given coefficients, positive or negative. I assume  $x = \frac{a+z}{b+z}$ :  $a$  and  $b$  being indeterminate quantities, and  $z$  a new unknown quantity. And it is to be observed, that the supposition of  $x = \frac{a+z}{b+z}$  is always possible, provided  $a$  be not equal to  $b$ : for if  $a$  be not equal to  $b$ , a value may be assigned to  $z$ , such, that the fraction  $\frac{a+z}{b+z}$  shall be equal to any number whatever, positive or negative. But if  $a = b$ , the value of  $\frac{a+z}{b+z}$  is not altered, whatever number  $z$  may denote.

HAVING substituted, and taken away the denominators, we get,

$$(a+z)^3 + A(a+z)^2(b+z) + B(a+z)(b+z)^2 + C(b+z)^3 = 0.$$

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THE terms of this expression are now to be evolved and arranged, according to the powers of  $z$ : which being done, we shall find,

$$\left. \begin{aligned} & a^3 + 3a^2 \times z + 3a \times z^2 + 1 \times z^3 \\ & + Aa^2b + 2Aab \times z + Ab \times z^2 \\ & \quad + Aa^2 \times z + 2Aa \times z^2 + A \times z^3 \\ & + Bab^2 + Bb^2 \times z \\ & \quad + 2Bab \times z + 2Bb \times z^2 \\ & \quad \quad + Ba \times z^2 + B \times z^3 \\ & + Cb^3 + 3Cb^2 \times z + 3Cb \times z^2 + C \times z^3 \end{aligned} \right\} = 0.$$

IN order to reduce this equation to our forms, we must equate three times the coefficient of  $z^3$  to the coefficient of  $z$ , either with the same or different signs: and also the coefficient of  $z^2$  to three times the absolute term, likewise with the same or different signs. For in the forms the coefficient of  $z^3$  and  $z$  are  $R$  and  $\mp 3R$ : and the coefficient of  $z^2$  and the absolute term are,  $-3r$  and  $\pm r$ . Now, in the transformed equation above, three times the coefficient of  $z^3$  is  $3 + 3A + 3B + 3C$ , which I write thus,  $(3 + 2A + B) + (A + 2B + 3C)$ : And in like manner, for three times the absolute term, I write  $(3a^3 + 2Aa^2b + Bab^2) + (Aa^2b + 2Bab^2 + 3Cb^3)$ . This being observed, we shall have these two equations for determining  $a$  and  $b$ :

$$\begin{aligned} \left. \begin{aligned} & 3 + 2A + B \\ & + A + 2B + 3C \end{aligned} \right\} &= \mp \left\{ \begin{aligned} & 3a^3 + 2Aa^2b + Bab^2 \\ & + Aa^2b + 2Bab^2 + 3Cb^3 \end{aligned} \right. \\ \left. \begin{aligned} & 3a + 2Aa + Ba \\ & + Ab + 2Bb + 3Cb \end{aligned} \right\} &= \mp \left\{ \begin{aligned} & 3a^3 + 2Aa^2b + Bab^2 \\ & + Aa^2b + 2Bab^2 + 3Cb^3 \end{aligned} \right. \end{aligned}$$

6. IT is manifest, from the manner in which I have written the two equations for determining  $a$  and  $b$ , that they depend upon

upon the two following more simple equations of the quadratic form :

$$3a^2 + 2Aab + Bb^2 = \mp (3 + 2A + B),$$

$$Aa^2 + 2Bab + 3Cb^2 = \mp (A + 2B + 3C).$$

FOR these two equations are no other than the two parts of the first of the preceding equations : and if we multiply the first of them by  $a$ , and the second by  $b$ , we shall have the two parts of the second of the preceding equations.

To determine  $a$  and  $b$ , I now write  $M = 3 + 2A + B$ ;  $N = A + 2B + 3C$ ; and  $ay = b$ : thus we have,

$$a^2 \times (3 + 2Ay + By^2) = \mp M,$$

$$a^2 \times (A + 2By + 3Cy^2) = \mp N.$$

MULTIPLY the first equation by  $N$ ; the second by  $M$ ; subtract the one from the other; and divide by  $a^2$ ; and there will result this equation for  $y$  :

$$(3N - AM) + 2(AN - BM)y + (BN - 3CM)y^2 = 0.$$

AND in this equation there is no ambiguity of signs.

IF we suppose  $y = 1$ , the equation last found is equivalent to the identical equation  $MN - MN = 0$ . One value of  $y$  is therefore unity. But this is precisely the case of  $a = b$ , which we have noticed above to be inapplicable to the present purpose. Nor is it to be wondered at that this value of  $y$  is of no use in the present inquiry : for it is manifest that it does not at all depend on the given quantities  $A, B, C, M$  and  $N$ , but merely on the peculiar manner in which the equation is constituted. We learn from hence, however, that there is always another value of  $y$ , whatever numbers  $A, B, C, M$  and  $N$  may denote; because every quadratic must have two roots or none at all.

QUADRATIC equations, with one root  $= 1$ , may be supposed to be thus generated:  $(y - 1) \times (my - n) = my^2 - (m + n)y + n = 0$ ,  
the

the two roots being 1 and  $\frac{n}{m}$ : Comparing this formula with our equation, we have  $n = 3N - AM$  and  $m = BN - 3CM$ : And so we get,

$$y = \frac{3N - AM}{BN - 3CM}.$$

Now,  $a^2 = \frac{\mp M}{3 + 2Ay + By^2}$  and  $b = a \times y$ : therefore,

$$a = (BN - 3CM) \times \mp \sqrt{\frac{\mp M}{3(BN - 3CM)^2 + 2A(3N - AM)(BN - 3CM) + B(3N - AM)^2}}$$

$$b = (3N - AM) \times \pm \sqrt{\frac{\mp M}{3(BN - 3CM)^2 + 2A(3N - AM)(BN - 3CM) + B(3N - AM)^2}}$$

THE quantities  $a$  and  $b$  are therefore found by a single extraction of the square root: and they have each two values, one positive, and the other negative. It is indifferent which of these two values of  $a$  and  $b$  be taken, provided they are correspondent values, so that  $b = a \times y$ . It is to be remarked too, that  $a$  and  $b$  have always real values, on account of the double sign prefixed to  $M$ ; for that sign is to be taken that will render the radical quantity positive. And it is to be carefully noted which of the two signs is necessary, that  $a$  and  $b$  may have real values: because on this depends the characteristic of the reduced equation, and whether it is to be referred to the first or second form, and, consequently, whether it has three roots, or only one. If the sign  $-$  is requisite that  $a$  and  $b$  may have real values, then the reduced equation will have the characteristic of the first form, and will have three roots. But if the sign  $+$  is requisite for that end, the reduced equation will have the characteristic of the second form, and will have only one root. All this is manifest from the statement in Art. 5.

7. THE rule, or law, according to which the preceding formulæ for  $a$  and  $b$  are constituted, is sufficiently simple and perspicuous; and the formulæ are therefore, in that respect, convenient for practice. But in examining the expression in the

PART I.

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denominator

denominator of the radical, I find that it is always divisible by  $M$ , the quantity in the numerator: and that thus the formulæ may be exhibited in another shape, having this advantage, that it will introduce smaller numbers in the arithmetical operations requisite for computing  $a$  and  $b$ .

I WRITE  $Q \times M = 3(BN - 3CM)^2 + 2A(BN - 3CM)(3N - AM) + B(3N - AM)^2$ , and evolving by actual multiplication

$$\begin{aligned} Q \times M = & 3B^2 \cdot N^2 - 18BC \cdot MN + 27C^2 \cdot M^2 \\ & + 6AB \cdot N^2 - 2A^2B \cdot MN \\ & - 18AC \cdot MN + 6A^2C \cdot M^2 \\ & + 9B \cdot N^2 - 6AB \cdot MN + BA^2 \cdot M^2 \end{aligned}$$

Now, the coefficient of  $N^2$  is,  $3B^2 + 6AB + 9B = 3B(B + 2A + 3) = 3B \times M$ : dividing therefore by  $M$ , we get,

$$\begin{aligned} Q = & 3B \times N^2 - (18BC + 2A^2B + 18AC + 6AB) \times N \\ & + (27C^2 + 6A^2C + BA^2) \times M. \end{aligned}$$

And if for  $N$  and  $M$  we substitute their values  $(A + 2B + 3C)$  and  $(3 + 2A + B)$ , we shall find,

$$Q = 12B^3 + 81C^2 - 54ABC + 12A^3C - 3A^2B^2$$

all the other terms destroying one another, except these five.

ALL the terms in this value of  $Q$ , being divisible by 3, I change  $Q$ , and put

$$Q = 4B^3 + 27C^2 - 18ABC + 4A^3C - A^2B^2$$

$$\text{or, } Q = (4B^3 + 27C^2) - 2AC(9B - A^2) + A^2(2AC - B^2)$$

And since  $3QM$  is now equal to the denominator of the radical in the preceding formulæ for  $a$  and  $b$ , we have the following new formulæ,

$$\begin{aligned} a &= \pm \frac{BN - 3CM}{\sqrt{+3Q}} \\ b &= \pm \frac{3N - AM}{\sqrt{+3Q}} \end{aligned}$$

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WHAT was remarked above, with regard to the double sign prefixed to M, is now to be applied to the double sign prefixed to Q.

8. THE preceding investigation supplies us with the following rule, or criterion, by which to determine, whether any proposed cubic equation has three real roots or not:

THE proposed equation will have three real roots, when the amount of the expression

$$(4B^3 + 27C^2) - 2AC(9B - A^2) + A^2(2AC - B^2)$$

is negative: But if this expression is positive, the equation will have only one real root: And, (as will afterwards be shewn), when the amount of the expression is  $= 0$ , the equation will have two equal roots.

9. HAVING now found  $a$  and  $b$ , if we substitute  $\frac{a+z}{b+z}$  for  $x$  in the proposed equation, we shall have an equation for  $z$  that will come under one of our two forms, and from which  $z$  (and consequently the root or roots of the proposed equation) may therefore be found. But such substitution is not necessary. For if we go back to Art. 5. and compare the transformed equation with the forms, we shall find  $R = 1 + A + B + C$ , and  $3R = M + N$ : also  $-3r = (3a + 2Aa + Ba) + (Ab + 2Bb + 3Cb) = Ma + Nb$ : Therefore

$$\frac{r}{R} = - \frac{Ma + Nb}{M + N}.$$

Whence the value or values of  $z$  are found by what is observed in Art. 4.

10. I SHALL now give the result of the whole analysis in the form of a general rule for the resolution of cubic equations, and add a few examples by way of illustration.

LET the proposed equation be

$$x^3 + Ax^2 + Bx + C = 0.$$

$$\begin{aligned}
 1. \text{ COMPUTE, } M &= 3 + 2A + B \\
 N &= A + 2B + 3C \\
 m &= BN - 3CM \\
 n &= 3N - AM,
 \end{aligned}$$

$$\text{And } Q = (4B^3 + 27C^2) - 2AC(9B - A^2) + A^2(2AC - B^2)$$

AND let it be carefully noted under which of the two following cases the equation comes :

CASE I. When  $Q$  is negative.

CASE II. When  $Q$  is positive.

To these two cases a third may be added, viz. when  $Q = 0$  : but of this case I shall treat in one of the following examples.

$$\begin{aligned}
 2. \text{ COMPUTE also, } a &= \frac{\pm m}{\sqrt{\mp 3Q}} \\
 b &= \frac{\pm n}{\sqrt{\mp 3Q}} = \frac{an}{m}
 \end{aligned}$$

$$\text{and } \tau = -\frac{Ma + Nb}{M + N}.$$

3. THEN, if the equation comes under case I : Find the angle  $\phi$ , of which the tangent is  $\tau$ , the radius being unity : take  $z = \tan \frac{\phi}{3}$ ,  $z = \tan \left( \frac{\phi}{3} + 120^\circ \right)$ , and  $z = \tan \left( \frac{\phi}{3} + 240^\circ \right)$  : And the three roots of the equation will be found, by substituting these values of  $z$  in the formula  $x = \frac{a + z}{b + z}$ .

4. BUT if the equation comes under Case II. we must compute

$$z = \frac{1 - \sqrt[3]{\frac{1 - \tau}{1 + \tau}}}{1 + \sqrt[3]{\frac{1 - \tau}{1 + \tau}}}$$

And the only root of the equation will be found, by substituting this value of  $z$  in the formula  $x = \frac{a + z}{b + z}$ .

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THE following examples are chiefly taken from Dr HUT-  
TON'S "*Tracts Mathematical and Philosophical.*" Tract 5.

II. EXAMPLE I. Let the proposed equation be

$$x^3 - 6x^2 + 11x - 6 = 0.$$

Here,  $A = -6$ ,  $B = +11$ ,  $C = -6$ : therefore

$$M = +2$$

$$N = -2$$

$$m = +14$$

$$n = +6$$

$$Q = -4$$

So that we have here Case I.

$$a = \frac{14}{2\sqrt{3}} = \frac{7}{\sqrt{3}}$$

$$b = \frac{6}{14} \times a = \sqrt{3}$$

$$\text{and } r = -\frac{2 \cdot \frac{7}{\sqrt{3}} - 2 \cdot \sqrt{3}}{2 - 2} = \infty$$

Now,  $\tan 90^\circ = \infty$ , and also  $\tan 270^\circ = \infty$ : And we may take  
either of these angles for  $\phi$ : Take  $\phi = 90^\circ$ , then,

$$z = \tan \frac{\phi}{3} = \tan 30^\circ = +\frac{1}{\sqrt{3}}$$

$$z = \tan \left( \frac{\phi}{3} + 120^\circ \right) = \tan 150^\circ = -\tan 30^\circ = -\frac{1}{\sqrt{3}}$$

$$z = \tan \left( \frac{\phi}{3} + 240^\circ \right) = \tan 270^\circ = \infty$$

$$\text{THEREFORE, } x = \frac{\frac{7}{\sqrt{3}} + \frac{1}{\sqrt{3}}}{\sqrt{3} + \frac{1}{\sqrt{3}}} = \frac{8}{4} = 2$$

$$x = \frac{\frac{7}{\sqrt{3}} - \frac{1}{\sqrt{3}}}{\sqrt{3} - \frac{1}{\sqrt{3}}} = \frac{6}{2} = 3$$

$$x = \frac{\frac{7}{\sqrt{3}} + \infty}{\sqrt{3} + \infty} = 1.$$

And so the three roots are 1, 2, and 3. •

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SINCE  $M + N = 3(1 + A + B + C)$ : it is obvious, that when  $M + N = 0$ , we shall also have  $1 + A + B + C = 0$ ; and for one root of the equation is unity. It is manifest too, that, in this case, the transformed equation for  $z$  becomes simply,  $3z^2 - 1 = 0$ : whence  $z = \pm \frac{1}{\sqrt{3}}$ ; and consequently the other

two roots of the equation are,  $x = \frac{a + \frac{1}{\sqrt{3}}}{b + \frac{1}{\sqrt{3}}}$ , and  $x = \frac{a - \frac{1}{\sqrt{3}}}{b - \frac{1}{\sqrt{3}}}$ .

Or, writing for  $a$  and  $b$ , their values,  $\frac{m}{\sqrt{-3}Q}$  and  $\frac{n}{\sqrt{-3}Q}$ , the two roots are,  $x = \frac{m + \sqrt{-3}Q}{n + \sqrt{-3}Q}$  and  $x = \frac{m - \sqrt{-3}Q}{n - \sqrt{-3}Q}$ .

12. EXAMPLE 2. Let there be proposed

$$x^3 - 6x^2 + 9x - 2 = 0.$$

Here  $A = -6$ ;  $B = +9$ ;  $C = -2$ : And hence,

$$M = 0$$

$$N = +6$$

$$m = +54$$

$$n = +18$$

$$Q = -108.$$

And we have here again Case I.

$$a = +3$$

$$b = +1$$

$$r = -1$$

Therefore  $\phi = \text{Arc. tan } -1 = 145^\circ$ , therefore,

$$z = \tan \frac{\phi}{3} = \tan 45^\circ = 1,$$

$$z = t. \left( \frac{\phi}{3} + 120^\circ \right) = t. 165^\circ = -t. 15^\circ = -(2 - \sqrt{3}),$$

$$z = t. \left( \frac{\phi}{3} + 240^\circ \right) = t. 285^\circ = -t. 75^\circ = -(2 + \sqrt{3});$$

$$\text{THEREFORE, } x = \frac{3 + 1}{1 + 1} = \frac{4}{2} = 2;$$

$$x = \frac{3-2+\sqrt{3}}{1-2+\sqrt{3}} = \frac{\sqrt{3}+1}{\sqrt{3}-1} = \frac{(\sqrt{3}+1)^2}{2} = 2 + \sqrt{3};$$

$$x = \frac{3-2-\sqrt{3}}{1-2-\sqrt{3}} = \frac{\sqrt{3}-1}{\sqrt{3}+1} = \frac{(\sqrt{3}-1)^2}{2} = 2 - \sqrt{3};$$

And the three roots are  $2$ ,  $2 + \sqrt{3}$ , and  $2 - \sqrt{3}$ .

13. I TAKE the following example to illustrate the case of two equal roots.

EXAMPLE 3. Let the equation be

$$x^3 - 7x^2 - 5x + 75 = 0.$$

Here  $A = -7$ :  $B = -5$ :  $C = +75$ . Therefore,

$$M = -16$$

$$N = +208$$

$$m = +2560$$

$$n = +512$$

$$Q = 0.$$

SINCE, then,  $Q = 0$ , it is manifest that  $a$ ,  $b$ , and  $\tau$  will be all infinitely great. But though  $a$  and  $b$  be infinite, it is to be remarked that  $\frac{a}{b} = \frac{m}{n}$ .

SINCE  $\tau = \infty$ , we have  $\phi = 90^\circ$ , or  $\phi = 270^\circ$ : Take  $\phi = 90^\circ$ : Then,

$$z = \tan \frac{\phi}{3} = \tan 30^\circ = \frac{1}{\sqrt{3}}$$

$$z = \tan \left( \frac{\phi}{3} + 120^\circ \right) = \tan 150^\circ = -\tan 30^\circ = -\frac{1}{\sqrt{3}}$$

$$z = \tan \left( \frac{\phi}{3} + 240^\circ \right) = \tan 270^\circ = \infty$$

WE have then, in the first place, for two roots,

$$x = \frac{a + \frac{1}{\sqrt{3}}}{b + \frac{1}{\sqrt{3}}}$$

$$x = \frac{a - \frac{1}{\sqrt{3}}}{b - \frac{1}{\sqrt{3}}}$$

And

And since  $a$  and  $b$  are infinitely great, it is manifest that these two roots are equal to one another, and each  $= \frac{a}{b} = \frac{m}{n}$ .

WE can infer nothing with regard to the value of the third root, derived from the infinite tangent, unless we can ascertain the proportion which that infinite tangent bears to the infinitely great quantities  $a$  and  $b$ . The general relation of  $\tau = \tan \phi$ , and  $z = \tan \frac{\phi}{3}$  is thus expressed  $\tau = \frac{3z - z^3}{1 - 3z^2}$ : and  $\tau$  becomes infinitely great; 1st, When  $1 - 3z^2 = 0$ ; 2dly, When  $z$  is infinitely great.

Now the values of  $z$ , derived from the equation  $1 - 3z^2 = 0$ , are  $z = +\frac{1}{\sqrt{3}}$ , and  $z = -\frac{1}{\sqrt{3}}$ : And these are precisely the values of  $z$  used above in determining the two equal roots.

AGAIN, we have  $\frac{\tau}{z} = \frac{3 - z^2}{1 - 3z^2}$ : And it is manifest that the greater  $z$  is, the nearer  $\frac{\tau}{z}$  approaches to  $\frac{1}{3}$ : so that, ultimately, when  $\tau$  and  $z$  are greater than any finite magnitudes, we have  $\frac{\tau}{z} = \frac{1}{3}$  and  $z = 3\tau$ . But  $\tau = \frac{Ma - Nb}{M + N}$ ; therefore,

$$z = \frac{-3Ma - 3Nb}{M + N} \text{ and } x = \frac{a + z}{b + z} = \frac{a - \frac{3Ma + 3Nb}{M + N}}{b - \frac{3Ma + 3Nb}{M + N}}. \text{ If now we}$$

write  $\frac{an}{m}$  for  $b$ , in this expression, the infinite quantity  $a$  may be thrown out by division, and we shall have the value of the root in finite quantities only. The expression being properly reduced, we shall have,  $x = \frac{2Mm + N(3n - m)}{M(3m - n) + 2Nn}$ .

WHEN, therefore,  $Q = 0$ , two roots are equal to one another, and each  $= \frac{m}{n}$ : And we have this formula by which to compute the third root,  $x = \frac{2Mm + N(3n - m)}{2Nn + M(3m - n)}$ .

APPLYING

APPLYING this rule to our example, we have:

for the two equal roots,  $x = \frac{m}{n} = \frac{2560}{512} = 5$

for the third root,  $x = \frac{-81020 - 212992}{-114688 + 212992} = \frac{-294912}{+98304} = -3$

14. EXAMPLE 4. Let the equation be

$$x^3 - 7x^2 + 18x - 18 = 0.$$

Here  $A = -7$ ;  $B = +18$ ;  $C = -18$ . Therefore,

$$M = +7$$

$$N = -25$$

$$m = -72$$

$$n = -26$$

$$Q = +72.$$

So that we have here Case II.

$$a = \frac{-72}{\sqrt{216}} = \frac{-36}{3\sqrt{6}}$$

$$b = \frac{-26}{\sqrt{216}} = \frac{-13}{3\sqrt{6}}$$

$$\tau = +\frac{73}{54\sqrt{6}}$$

$$\text{Therefore, } \sqrt[3]{\frac{1-\tau}{1+\tau}} = \sqrt[3]{\frac{54\sqrt{6}-73}{54\sqrt{6}+73}} = \frac{2\sqrt{6}-1}{2\sqrt{6}+1}$$

$$\text{HENCE } z = \frac{1 - \sqrt[3]{\frac{1-\tau}{1+\tau}}}{1 + \sqrt[3]{\frac{1-\tau}{1+\tau}}} = \frac{1}{2\sqrt{6}}$$

$$\text{and } x = \frac{-\frac{36}{3\sqrt{6}} + \frac{1}{2\sqrt{6}}}{-\frac{13}{2\sqrt{6}} + \frac{1}{2\sqrt{6}}} = \frac{-72+3}{-26+3} = \frac{-69}{-23} = +3.$$

And 3 is the only root of the equation.

15. WHEN  $\tau$  is a furd as  $\frac{p}{q\sqrt{r}}$ , the value of  $z$  (in Case II.) always involves radicals of this form,  $\sqrt[3]{\frac{q\sqrt{r}-p}{q\sqrt{r}+p}}$ ; out of which the root may sometimes be extracted; and so the value of  $z$  will be expressed by a furd of the same kind as  $\tau$ .

PART I.

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THE method I have followed to find when this can be done, being very fimple and easy in practice, I shall here briefly describe it.

$$\text{WE have } \tau = \frac{3z + z^3}{1 + 3z};$$

write  $\frac{p}{q\sqrt{r}}$ , for  $\tau$ ,  $\frac{\mu}{v\sqrt{r}}$ , for  $z$ : then

$$\frac{p}{q\sqrt{r}} = \frac{3r\mu v + \mu^3}{(rv^3 + 3\mu^2v)\sqrt{r}},$$

whence these two equations are formed.

$$p = 3r\mu v + \mu^3$$

$$q = rv^3 + 3\mu^2v,$$

from which it is manifest, that  $\mu$  is a divisor of  $p$ , and  $v$  a divisor of  $q$ . I seek then amongst the divisors of  $p$  for a number  $\mu$ , and amongst the divisors of  $q$  for a number  $v$ , that will satisfy the two equations above: or rather, that will satisfy these two following,

$$v^2 = \frac{p - \mu^3}{3r\mu},$$

$$\mu^2 = \frac{q - r^3}{3v}.$$

If two such numbers are to be found amongst the divisors of  $p$  and  $q$ , then will  $z = \frac{\mu}{v\sqrt{r}}$ : but if not, we are to conclude that the value of  $z$  cannot be expressed this way.

THUS, in the last example  $\tau = \frac{73}{54\sqrt{6}}$ , it is manifest that 73 admits no divisor but 1: therefore  $\mu = 1$ , and on trial I find  $v = 2$ , which two numbers satisfy the two equations, and therefore  $z = \frac{1}{2\sqrt{6}}$ .

16. THE same method applies also to Case I. For, in this case,

$$\tau = \frac{3z - z^3}{1 - 3z^2}$$

and



and substituting  $\frac{p}{q\sqrt{r}}$  for  $\tau$ , and  $\frac{\mu}{v\sqrt{r}}$  for  $z$ , we derive these two equations,

$$p = 3r\mu v^2 - \mu^3$$

$$q = rv^3 - 3\mu^2 v,$$

whence it is manifest that  $\mu$  is a divisor of  $p$ , and  $v$  a divisor of  $q$ , as before. It will be easier for trial to write the equations thus :

$$v^2 = \frac{p + \mu^3}{3r\mu}$$

$$\mu^2 = \frac{rv^3 - q}{3v}$$

AND let it be observed, that we may here give to  $\mu$  and  $v$  any signs consistent with the condition, that  $\frac{p + \mu^3}{3r\mu}$  and  $\frac{rv^3 - q}{3v}$  (the values of  $v^2$  and  $\mu^2$ ) are positive numbers.

It is to be remarked too, that, in this case,  $z$  has three values. If, however, we can find one value this way, the two others are readily obtained. For if  $v$  be one value of  $z$ , the two other values are  $\frac{v + \sqrt{3}}{1 - v\sqrt{3}}$  and  $\frac{v - \sqrt{3}}{1 + v\sqrt{3}}$ : because these values are the tangents of two arches that differ from the arch of which  $v$  is the tangent by  $120^\circ$  and  $240^\circ$ .

THOUGH this is a matter more curious than useful, I shall add one more example for the sake of illustration.

LET the equation be

$$x^3 - 39x^2 + 479x - 1881 = 0.$$

Here  $A = -39$ ;  $B = +479$ ;  $C = -1881$ : And

$$M = + 404$$

$$N = - 4724$$

$$m = + 16976$$

$$n = + 1584$$

$$Q = - 25600$$

And the equation belongs to Case I.

$$a = \frac{16976}{160\sqrt{3}} = \frac{1061}{10\sqrt{3}}$$

$$b = \frac{1584}{160\sqrt{3}} = \frac{99}{10\sqrt{3}}$$

$$r = -\frac{39032}{43200\sqrt{3}} = -\frac{4879}{5400\sqrt{3}}$$

To find whether  $z$  can be expressed by a surd as  $\frac{\mu}{v\sqrt{r}}$ : I write  $p = -4879$ ,  $q = 5400$ , and  $r = 3$ ; and I have these two equations,

$$v^2 = \frac{-4879 + \mu^2}{9\mu}$$

$$\mu^2 = \frac{3v^2 - 5400}{3v}$$

I find, that 7, 17, and 41, are divisors of 4879. It is evident that + 7 would give  $v^2$  negative: I therefore try - 7, and it does not succeed: Neither does + 17; but trying - 17, I find  $v^2 = 64$ , and consequently  $v = \pm 8$ : And - 8 is found to succeed in the other equation: therefore  $\mu = -17$ ,  $v = -8$ , and  $z = \frac{17}{8\sqrt{3}}$ .

$$\text{We have then, } z = \frac{17}{8\sqrt{3}} = v$$

$$z = \frac{v - \sqrt{3}}{1 + v\sqrt{3}} = \frac{-7}{25\sqrt{3}}$$

$$z = \frac{v + \sqrt{3}}{1 - v\sqrt{3}} = \frac{41}{-9\sqrt{3}}$$

$$\text{Therefore, } x = \frac{\frac{1061}{10\sqrt{3}} + \frac{17}{8\sqrt{3}}}{\frac{99}{10\sqrt{3}} + \frac{17}{3\sqrt{3}}} = \frac{8488 + 170}{792 + 170} = \frac{8658}{962} = 9$$

$$x = \frac{\frac{1061}{10\sqrt{3}} - \frac{7}{25\sqrt{3}}}{\frac{99}{10\sqrt{3}} - \frac{7}{25\sqrt{3}}} = \frac{5305 - 14}{495 - 14} = \frac{5291}{481} = 11$$

$$x = \frac{\frac{1061}{10\sqrt{3}} - \frac{41}{9\sqrt{3}}}{\frac{99}{10\sqrt{3}} - \frac{41}{9\sqrt{3}}} = \frac{9549 - 410}{891 - 410} = \frac{9139}{481} = 19.$$

And so the three roots are, 9, 11, and 19.

END OF PART FIRST.

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VI. REMARKS *on a Mixed Species of EVIDENCE in MATTERS of HISTORY: With an EXAMINATION of a new HISTORICAL HYPOTHESIS, in the Mémoires pour la Vie de PETRARQUE, by the Abbé de SADE. By ALEXANDER FRASER TITTLER, ESQ. F. R. S. EDIN. Judge-Advocate of North Britain.*

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[Read by Mr PLAYFAIR, August 21. 1797.]

IN matters of historical research, there is a kind of circumstantial evidence which arises from the combination of known or authenticated facts, with critical argument on the import of doubtful passages of authors, which the reasoner endeavours to interpret, by bringing together, comparing, and making the one illustrate the other; so as to draw from the whole a degree of positive and certain information, which those authenticated facts are not of themselves sufficient to convey, and which those passages, taken separately, are incapable of furnishing. This complex species of evidence, it must be owned, is, with respect to its power of conviction, much inferior to that which arises from the ordinary proofs on which authentic history depends; for example, the testimony of actual witnesses to the facts related; or the positive information of authors, derived

from clear and well authenticated records: But, at the same time, as in matters of history we have not always that best kind of evidence on which to found our belief, we are from necessity often compelled to resort to this inferior, circumstantial and analogical species of probation, in order to form to ourselves a rational creed on many matters of doubt, on which the mind is unwilling to rest in absolute uncertainty.

THIS latter species of evidence, too, has its recommendation, from that pleasing exercise which it gives to the reasoning powers: for there is no mental occupation which demands more ingenuity, if we construct ourselves this artificial fabric of argument, or requires more judgment, if we attempt to analyze and examine its foundations when reared by another. The structure of an hypothesis of our own is a delightful occupation to the mind; and a pleasure, perhaps very little inferior to this, is the critical examination of an hypothesis framed by another. We proceed to either undertaking with a zeal, resembling that which is felt by the adventurer on a voyage of discovery. We persevere in it with an ardour which increases with the difficulties we encounter. Like the adventurer, too, we find our chief reward in the research itself, whatever may be the actual value or real importance of its ultimate object.

OF the truth of these observations I have myself had full experience, while, at a season when I thought myself warranted in seeking an amusing exercise of the mind, I began to read over, with some attention, an elaborate work that I had formerly but hastily perused, the *Mémoires pour la Vie de PETRARQUE*, by the Abbé de SADE; a composition which affords one of the most remarkable examples of an historical hypothesis, supported by that compound species of evidence I have mentioned, which consists in the combination of certain authenticated facts, with critical reasoning on the import of various doubtful and contending authorities. As the examination of this hypothesis, and the

the evidence on which it is built, has afforded me considerable pleasure, while, at the same time, I have flattered myself that the result of this examination has been a complete detection of its insufficiency, I am prompted to submit my remarks to the consideration of this Society, which embraces, in its comprehensive institution, equally the subjects of philosophical research and of historical and critical disquisition.

IN the examination of this hypothesis, I laid down to myself certain rules, which, as they are of a general nature, may, as I imagine, be applied with propriety to all investigations of a similar kind, where the evidence is of that compound species I have mentioned. These rules are the following:

I. WHERE a doubtful fact is to be ascertained, by bringing together, comparing and weighing the sense of various passages of an author's writings, the construction put on *ambiguous* expressions ought to be such as is consonant with the sense of those passages or expressions, which, on the same subject, are *plain* and *unambiguous*.

II. WHERE a person's *character* and *manner of thinking*, *feeling*, or *acting*, are clear, from the general tenor of his life and writings, no interpretation ought to be given to doubtful passages of those writings, which contradicts, or is inconsistent with, such *character*, *sentiments*, and *conduct*.

III. WHERE many passages concur to establish the belief of the disputed fact, a single passage, though apparently contradictory to that supposition, must not be allowed weight, if it is possible to give it an explanation consistent with that opinion which is better supported.

IV. IN such a case, where many passages concur to establish the belief of a certain fact, and there appear one or two passages in apparent contradiction to that belief, there is room to suspect  
either

either an *error of transcription or typography*; or, if such supposition is excluded, *interpolation or fabrication*.

V. IN the supposition of interpolation or fabrication, there must of necessity be included a cogent and adequate *motive*; and therefore, where such motive is utterly wanting, the supposition is not to be indulged.

VI. WHERE this motive is apparent, the presumption of *falsehood* is in proportion to the strength of the motive, the facility of executing the deception, and the weight of the opposing evidence.

VII. WHERE a passage is suspected of interpolation or fabrication, it is most material to attend to the sense of the *context*, or what immediately precedes and follows the passage in dispute; as its *consonancy or dissonancy* is strong matter of corroboration.

IF these rules of evidence are well founded, they will afford a just criterion for the decision of all questions of historical controversy, where the evidence is of a compound, circumstantial and presumptive nature; and where our belief is the consequence not of authority but of argument. Of such a nature is that hypothesis of the Abbé de SADE, which I shall now proceed to examine.

THE literary world owes very high obligations to FRANCIS PETRARCH; and it has been laudably zealous in repaying them by a grateful tribute of admiration and encomium. There is no character among the moderns whose talents have been more the subject of panegyric, both among his cotemporaries and with posterity; nor is there any whose life has so frequently employed the pen of the biographer. The Baron de la BASTIE, one of the latest of those biographers, enumerates at least fourteen, and most of these authors of some reputation, who have professedly written biographical accounts of this eminent man;

not

not to mention a still more numerous class, who have incidentally treated of his life and literary character in critical dissertations, journals, and memoirs of the state of literature of his age and country.

AMIDST this variety of research, it is natural to suppose that much has been brought to light, and, it is even a reasonable suspicion, that where there has been such ardour of investigation, there can remain but little subject of new discovery. So thought M. de la BASTIE, when, in the years 1740 and 1741, he published his excellent Account of the Life of PETRARCH, in the *Mémoires de l'Académie des Inscriptions et Belles Lettres* \*. He was not aware, that within a few years from the date of his own publication, the world was to receive a work, containing at least ten times the quantity of all that had ever been written on the subject of PETRARCH, and professing to be only *materials* for the composition of a Life of this remarkable man. The work I here allude to, is *Mémoires pour la Vie de PETRARQUE*, by the Abbé de S A D E; a book which has met with considerable approbation from the public, and which merits the praise it has obtained; as, along with a very minute detail of the life of PETRARCH himself, and a very full account of his writings, it contains much curious and instructive information relative both to the literary and political history of the times which it commemorates. In a work which is extended to three large volumes in quarto, and which has been the fruit of the most elaborate research, not only into all the printed works illustrative of the manners and history of the 14th century, but into public records and the archives of private families, it cannot be denied, that much new light has been thrown on the principal object of that research, the *history* and character of PETRARCH. But it will be a matter of regret to many readers, that the con-

sequence

\* Vols. xv. and xvii.

sequence of these elaborate inquiries, should be a diminution of that esteem which the world has hitherto entertained for this eminent man, in the most important point of his character, his *morals* \*.

THE works of PETRARCH give evidence of his abilities as a Politician, Theologian, and Philosopher ; and it is in these characters that he appears chiefly to have been distinguished by his cotemporaries ; but it is not on these foundations that the lasting structure of his fame has been reared. It is to those incomparable verses, in which he has celebrated the accomplishments and bewailed the fate of the beautiful LAURA, that PETRARCH has been indebted for his permanent reputation. The history of the poet's passion for his lovely mistress, must ever be regarded as forming the most interesting portion of his annals. His character, in fact, took its tone from that predominant affection, which influenced his studies, his habits of life, and all his pursuits and occupations. A love so pure, so ardent, and so lasting, is difficult to be paralleled in the history of human nature. PETRARCH was the passionate admirer of LAURA for twenty-one years, while she was in life ; and with unabated ardour of affection, he is said to have bewailed her loss for twenty-six years after her death.

THE works of the poet himself bear the strongest testimony that this passion, so remarkable both in its fervency and duration, was an honourable and virtuous flame. PETRARCH aspired to the happiness of being united to LAURA in marriage. We have reason to believe, from the evidence of his own writings, that the heart of LAURA was not insensible to his passion ; and, although the term of his probation was tedious and severe, he had grounds for a hope, approaching to confidence, that he

was

\* Of this work of the Abbé de SADE, Mrs DOBSON has given a very amusing epitome, in her *Life of PETRARCH*, 2 vols. 8vo.



was at last to attain the end of his wishes. Such are the ideas that we must entertain, from the writings of the poet himself, of the nature and object of his passion; and such has been the uniform and continued belief of the world with regard to it, from his own days to the present. At length comes into the field a hardy but most uncourteous Knight, who, with a spirit very opposite to that of the heroes of chivalry, blasts at once the fair fame of the virtuous LAURA, and the hitherto unfulfilled honour of her lover; and, proudly throwing down his gauntlet of defiance, maintains, that LAURA was a married woman, the mother of a numerous family; that PETRARCH, with all his professions of a pure and honourable flame, had no other end in his unexampled assiduity of pursuit, than what every libertine proposes to himself in the possession of a mistress; and that the lovely LAURA, though never actually unfaithful to her husband's bed, was sensible to the passion of her *Cicisbeo*, highly gratified by his pursuit, and, while she suffered on his account much restraint and severity from a jealous husband, continued to give him every mark of regard which, without a direct breach of her matrimonial vow, she could bestow upon him. Such is the hypothesis of the author of the *Mémoires pour la Vie de PETRARQUE*, on the subject of the loves of PETRARCH and of LAURA; and the establishment of this hypothesis, so injurious to the honour of both, is, in fact, the main scope of that most elaborate work.

THE principle of sympathy is a noble part of the constitution of the human mind; and is, perhaps, the basis of all the social affections. In forming our opinions of the characters and conduct of other men, we involuntarily place ourselves in their situation, and we judge of them as we should wish to be judged ourselves in similar circumstances. Hence, in every doubtful case, where the conduct of another is found to admit of opposite constructions, a candid mind will ever give its decision on the side of

virtue and of honour. Hence, likewise, the warmest indignation arises in every ingenuous breast, on observing in others a violation of this rule of candour, in a propensity to form unfavourable opinions of conduct or of character; we are prompted eagerly to scrutinize the foundations of such illiberal opinions; and we conceive it a duty we owe to virtue and to honour, a task enjoined us by the respect due to our common nature, to refute the calumny, expose the artifices of the aggressor, and restore the injured to his just estimation.

PREVIOUSLY to the appearance of the work of the Abbé de SADE, those authors who had written the Life of PETRARCH, had, in treating of LAURA, universally acknowledged, that much uncertainty prevailed with regard to the real name, family and rank of this celebrated personage. In general, however, we find but two different opinions on this subject. The one is, that her parents were of an honourable family in Provence; and that her father\* resided at a small country seat or village in the territory of Avignon, near to the sources of the Sorga: the other, that she was sprung from the house of SADE, a daughter of that family, which is of an ancient date, and considerable rank in the city of Avignon. The former of these opinions, which has been adopted by almost all the Italian authors, is founded on a variety of passages in the writings of the poet himself, of which I shall afterwards take particular notice. The latter opinion, which

\* To this gentleman VELUTELLO has given the name and title of HENRI CHIABAU, Lord of Cabrieres. This, however, is founded on too slender an authority to entitle it to credit. It is, in fact, only a conjecture of VELUTELLO himself, who was at pains to search the baptismal register of the parish in which Vaucluse is situate; and, finding that a child of the name of LAURA was registered as being born to HENRI CHIABAU, Lord of Cabrieres, on the 4th of June 1314, he thence concluded, there being no other registration of that name which could possibly apply to the object of his research, that this, for certain, was the mistress of PETRARCH.

which is that which chiefly prevails at Avignon, where the poet passed a considerable part of his life, is founded on the following circumstances.

IN the Ambrosian Library at Milan, there is preserved a manuscript copy of VIRGIL, which is said to have been the property of PETRARCH; and on the margins of which are many notes, alleged to be in the handwriting of the poet. One of these, which is written on the first page of the MS. is in the following words :

\* " LAURA, propriis virtutibus illustris, et meis longum celebrata carminibus, primum oculis meis apparuit, sub primum adolescentiæ meæ tempus, anno Domini 1327, die 6. mensis Aprilis, in Ecclesia Sanctæ CLARÆ Avinioni, horâ matutinâ ; et in eadem civitate, eodem mense Aprilis, eodem die sexto, eadem horâ primâ, anno autem 1348, ab hac luce lux illa subtracta est ; cum ego fortè tunc Veronæ essem, heu fati mei nescius ! rumor autem infelix per literas LUDOVICI mei, me Parmæ reperit, anno eodem, mense Maio, die 19. manè. Corpus illud castissimum atque pulcherrimum, in loco *Fratrum Minorum* repositum est, ipso die mortis, ad vesperam. Ani-

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" mam

\* TRANSLATED.

" LAURA, illustrious by the virtues she possessed, and celebrated during many years by my verses, appeared to my eyes, for the first time, on the 6th day of April, in the year 1327, at Avignon, in the church of St Claire, at 6 o'clock in the morning. I was then in my early youth. In the same town, on the same day, and at the same hour, in the year 1348, this light, this sun withdrew from the world. I was then at Verona, ignorant of the calamity that had befallen me. A letter I received from my LUDOVICO, on the 19th of the following month, brought me the cruel information. Her body, so beautiful, so pure, was deposited, on the day of her death, after vespers, in the church of the Cordeliers. Her soul, as SENECA has said of AFRICANUS, I am confident, returned to heaven, from whence it came. For the purpose of often dwelling on the sad remembrance of so severe a loss, I have written these particulars in a book that comes frequently under my inspection. I have

“mam quidem ejus, ut de AFRICANO ait SENECA, in cœlum  
 “unde erat rediisse mihi persuadeo. Hoc autem ad acerbam rei  
 “memoriam, amara quâdam dulcedine scribere visum est, hoc  
 “potissimum loco qui sæpe sub oculis meis redit; ut cogitem  
 “nihil esse debere quod amplius mihi placeat in hac vita, et  
 “effracto majori laqueo, tempus esse de Babylone fugiendi,  
 “crebra horum inspectione, ac fugacissimæ ætatis æstimatione  
 “commoneat. Quod præviâ DEI gratiâ facile erit, præteriti  
 “temporis curas supervacuas, spes inanes, et inexpectatos exitus  
 “acriter et viriliter cogitanti.”

THE evidence of this note, supposing it authentic, positively fixes the burial-place of PETRARCH's LAURA to have been in the church of the *Fratres Minores*, or Cordeliers, at Avignon. At the distance of near two hundred years from the death of LAURA, (anno 1533,) a Florentine gentleman, of the name of MANELLI, being at that time at Avignon, amused himself in making researches into every particular relative to the history of PETRARCH and of LAURA; and, with the aid of two others, MAURICE de SEVES\* and a M. BONTEMPS, who were instiga-

have thus prepared for myself a pleasure mingled with pain. My loss, ever present to my memory, will teach me, that there is no longer any thing in this life which can afford me delight: That it is now time that I should renounce Babylon, since the chain which bound me to it with so tender an attachment, is broken. Nor will this, with the assistance of Almighty GOD, be difficult. My mind, turning to the past, will set before me all the superfluous cares that have engaged me; all the deceitful hopes that I have entertained; and the unexpected and afflicting consequences of all my projects.”

\* PROBABLY MAURICE SCEVE, a French poet, cotemporary with CLEMENT MAROT, of whose composition there are some pieces in the 4to edition of the works of MAROT, published at the Hague, 1731, by the Abbé LANGELET du FRESNOY, (under the fictitious name of the Chevalier GORDON de PERCEL).

ted by a like curiosity, examined every quarter of this church of the Cordeliers, in the hope of tracing some memorial of LAURA, whom, on the evidence of the manuscript note on *Virgil*, they believed to have been there interred. In one of the chapels of that church, called *Capella della Croce*, in which was the burial-place of the family of SADE, they found a large flat stone, which bore no inscription whatever. This, if it afforded no indication of being the grave of LAURA, at least gave no proof to the contrary; they therefore opened the grave in quest of further evidence. At first they perceived nothing but earth, intermixed with small bones, among which was an entire jaw-bone. Examining, however, more minutely, they discovered a little casket of lead, fastened with a brass wire, on opening which they found a piece of parchment, folded, and sealed with green wax, together with a medal of bronze, on the one side of which was the figure of a very little woman, (*figura d'una donna picciolissima*), in the attitude of uncovering her bosom with both her hands; and around, in the way of legend, were only these four letters, M. L. M. I.

IN endeavouring to explain this inscription, it occurred to MAURICE de SEVES, that the four letters might probably be thus interpreted, *Madonna LAURA morta jace*. This was plainly nothing more than conjecture; and, had the tomb contained no other evidence of a more decisive nature, all hitherto discovered had been of little consequence to ascertain the object of inquiry. The sealed parchment was, therefore, next examined; and, although it is owned that the writing was at first quite illegible, the characters being so defaced, (as well they might, after lying two hundred years in the dissolved materials of a human body), yet MAURICE de SEVES, by examining it exposed to the strong rays of the sun, is said to have at last made out distinctly

distinctly the following sonnet, of which he took a fair transcript\* :

*Qui riposan quei caste e felici ossa  
Di quella alma gentile e sola in terra.  
Aspro't dur sasso hor ben teco hai soterra  
El vero honor la fama e belta scossa.  
Morte ha del verde Lauro svelta e mossa  
Fresca radice, e il premio de mia guerra  
Di quattro lustri ; e piu se anchor non erra  
Mio penser tristo ; e il chiude in pochi fassa.  
Felice pianta : in borgo de Avignone  
Nacque e mori : e qui con ella jace  
La penna, el stil, l'inchostro e la ragione.  
O delicate membra, O viva face  
Che anchor me cuoci e struggi, inginocchione  
Ciascun preghi il SIGNOR te accepti in pace.*

THIS

\* THE sonnet is here given exactly, as to orthography and punctuation, from the copy which the Abbé de SADE says he took from the original.

THUS almost literally translated :

Here now repose those chaste, those blest remains  
Of that most gentle spirit, sole in earth !  
Harsh monumental stone, that here confin'd  
True honour, fame and beauty, all o'erthrown !  
Death has destroy'd that Laurel green, and tort  
Its tender roots ; and all the noble meed  
Of my long warfare, passing (if aright  
My melancholy reckoning holds) four lustres.  
O happy plant ! Avignon's favour'd soil  
Has seen thee spring and die ; — and here with thee  
Thy poet's pen, and muse, and genius lies.  
O lovely, beauteous limbs ! O fire divine,  
That even in death hast power to melt the soul !  
Heaven be thy portion, peace with God on high !

THIS sonnet is apparently written in the person of PETRARCH; but being, in the judgment of BEMBO, BECCATELLI, MURATORI, and the best of the Italian critics, very inferior, in point of merit, to the other compositions of the poet, they have supposed it the work of another, who had here assumed his character, in composing an epitaph in honour of LAURA. It is remarked, from the evidence of the note on VIRGIL, that PETRARCH was at Parma at the time when LAURA was interred at Avignon; and therefore, that this inscription, though written under his character, could not possibly be of his composition. If, however, such an inscription was actually found in 1533, in the tomb of the church of the Cordeliers, whoever was its author, it would seem very clearly to indicate, that this was in reality the grave of PETRARCH'S LAURA; and the place of her interment, being a chapel erected by the house of SADE as a burial-place for all of their family, a strong confirmation thence arises, of the tradition current at Avignon, that LAURA was of that family; and a reasonable foundation seems to be laid for that hypothesis of the author of the *Mémoires*, that she was the same person with LAURA de NOVES, who was married to HUGH de SADE, and from whom the whole of the branches of that family now existing are lineally descended.

THERE are, however, many circumstances that tend to bring into doubt, or rather that seem entirely to confute, this fundamental fact, that LAURA either died or was buried at Avignon.

IF we admit the evidence of the note on VIRGIL, LAURA was born at Avignon, and died in the same place. The works of the poet, indeed, contain the most positive information that LAURA died in the same place where she was born, and where she had passed the greatest part of her life; but they likewise furnish evidence that this place was not Avignon, but some small village or country-seat in the territory of Avignon, near to the source of the *Sorga*, or the fountain of *Vaucluse*.

IN the *Trionfo della Morte*, Part 2. the poet feigns that LAURA, on the night after her death, appeared to him in a vision: and, in the course of a long conversation, in which she acknowledges, that she had ever felt for him a mutual passion, and endeavours to satisfy him, that every singularity of her conduct, however harshly he might at the time have judged it, was prompted by the sincerity of her affection for him; she says in one passage,

*In tutte l'altre cose assai beata,  
In una sola a me stessa dispiacqui;  
Che'n troppo umil terren mi trovai nata:  
Duolmi ancor veramente ch'io non nacqui  
Almen più presso al tuo fiorito nido;  
Ma assai fu bel paese ov' io ti piacqui.*

"Fortunate enough I was in other respects: this only I regretted, that the place of my birth was too humble: at least I had cause to repine at this circumstance, that it was not nearer to the beautiful country of thy nativity. Yet that region was indeed sufficiently beautiful where I had the happiness to please thee." It was impossible that LAURA could have termed the city of Avignon *umil terreno*, or that she could have been ashamed of it as the place of her birth. At that time Avignon was the Papal residence, and one of the most splendid cities in the south of Europe; a city, indeed, where luxury and corruption of manners had attained to such a height, that PETRARCH himself characterizes it by the epithet of the *Gallie Babylon* \*.

IN the fourth sonnet of the 1st Part of his *Sonetti e Canzoni*, the poet has the following remarkable allusions, which may per-

haps

\* EPIST. lib. sine tit. Ep. 16.



haps be thought to border a little on impiety. Divine Providence, he observes, has thought fit to display its wonders, by choosing its most illustrious instruments, either from a servile condition, or from a low and obscure place of origin. Of this he gives for examples, the mean occupations of the Apostles, the obscurity of the birth-place of our Saviour, who, disdaining imperial Rome, chose an inconsiderable town of Judæa for the place of his nativity; and lastly, the humble origin of the matchless LAURA, that resplendent sun of beauty, who rose upon the world from a small obscure village:

*Ed or di picciol borgo un sol n' ha dato,*

*Tal, che natura e 'l luogo si ringrazia;*

*Onde sì bella donna al mondo nacque.*

Son. 4. Part. 1.

THE situation of this *picciol borgo*, or small village, is likewise distinctly pointed out. It was in the neighbourhood of the hills that rise above the fountain of *Vaucluse*, the spring of the *Sorga*. The poet sends a present to a friend, of two birds which he had caught, and he accompanies the gift with a sonnet, in which the birds are supposed thus to address the person to whom they are sent:

*A piè de' colli, ove la bella vèsta*

*Prese delle terrene membra pria*

*La donna, che colui ch' a te ne 'nvia,*

*Spesso dal sonno lagrimando desta;*

*Libere in pace passavam, &c.*

Son. 8. Part. 1.

" At the foot of those hills where that fair nymph was born,  
" who oft causes him who sends this present to pass the sleepless  
" night in tears, we once enjoyed the sweets of liberty," &c.

IN the 37th sonnet of the second Part, PETRARCH, addressing himself to the spirit of LAURA, thus expresses himself:

*Mira 'l gran sasso donde Sorga nasce,  
E vedravi un che sol tra l'erbe e l'acque  
Di tua memoria e di dolor si pasce:  
Ove giace 'l tuo albergo e dove nacque  
Il nostro amor.*

“Behold,” says he, “yonder great rock from whence the Sorga springs, and there thou wilt see a solitary being, who, amidst the green fields and streams, feeds on thy remembrance and on his own sorrows. ’Twas there thy habitation lay; and there our loves began.”

IN the 155th sonnet of the 1st Part, (*Almo Sol*), the poet, addressing himself to the sun, complains, that when his light is withdrawn, and the night comes on, he is deprived of what he most delights in, the shadow that falls from that low hill, “where sparkles that sweet fire; where from a slender twig the beautiful laurel grew;” and he laments, that the darkness hides from his eyes “that blessed spot, where, with its mistress, his heart for ever dwells.”

*O sole,—  
—Fuggendo mi toi quel ch' i' più bramo:  
L' ombra che cade da quell' umil colle,  
Ove sfavilla il mio soave fuoco,  
Ove 'l gran lauro fu picciola verga;  
Crescendo, mentr' io parlo; a gli occhi tolle  
La dolce vista del beato loco,  
Ove 'l mio cor con la sua donna alberga.*

So, in the 40th sonnet of the 2d Part, the poet says,

*Quella per cui con Sorga ho cangiata Arno,—*

a passage clearly marking, that his preference for the Sorga arose from LAURA's residing on its banks.

THE 17th Canzone, Part. 1., (*Di pensier in pensier*), furnishes a similar inference:

*Oltra quell' alpe,  
Là, dovè 'l ciel è più sereno e lieto,  
Mi rivedrai fivr' un ruscel corrente,  
Ove l'aura si sente  
D' un fresco ed odorifero laureto:  
Ivi è 'l mio cor, e quella che 'l m' invola.*

“ Beyond those hills where the air is mild and serene; beside  
“ the rushing brook, where the gale wafts from yon fresh laurel's  
“ leaves its rich perfume; 'tis there she dwells who rest me of  
“ my heart.” On this passage GESUALDO remarks, in a note,  
“ Era presso al fiume, ov' albergava Madonna LAURA, che si  
“ dolce spirava.” “ It was near the river where lay the residence  
“ of LAURA, that laurel whose fragrance was to him so delight-  
“ ful.”

IN the 52d sonnet of the 2d Part, the poet thus describes his feelings on returning to Vacluse, after the death of LAURA:

*Sento l'aura mia antica; e i dolci colli  
Veggio apparir onde 'l bel lume nacque  
Che tenne gli occhi miei mentr' al ciel piacque  
Bramosi e lieti; or li tien tristi e molli.—  
Vedovè l'erbe, e torbide son l'acque;  
E voto e freddo 'l nido in ch' ella giacque.—*

“ Once more I breathe that dear accustomed air: Once more I  
“ view those beautiful hills, whence that resplendent light arose  
“ which once gave joy to these eyes; while heaven so pleased to  
“ bless me with her sight; but now, alas! has steeped them for

“ ever in tears.—Widowed are those green fields,—and turbid  
 “ is that stream,—and void and cold the nest in which she  
 “ lay.”

MANY passages of the poet's writings likewise very clearly indicate that LAURA died in the same place where she was born, and where she had passed the greatest part of her life.

THUS, in the 53d sonnet of the 2d Part :

*È questo 'l nido in che la mia Fenice  
 Mise l' aurate e le purpuree penne ;  
 Che sotto le sue ali il mio cor tenne.—  
 E me lasciato hai quì misero e solo,  
 Tal, che pien di duol sempre al loco torno  
 Che per te consacrato onorò e colò.  
 Veggendo a' colli oscura notte intorno  
 Onde prendesti al ciel l' ultimo volo.—*

“ Here is the nest in which my Phœnix lay, and couched her  
 “ golden and empurpled wings ;—and here she left me miserable  
 “ and alone. Thus, for ever a prey to grief, I turn me to that  
 “ dear spot of earth which she has consecrated, and which, on  
 “ that account, I venerate and honour : I view those hills, now  
 “ dark and desolate, from whence she winged her flight to  
 “ heaven.”

So, likewise, in that beautiful sonnet on his return to Vaucluse, *Valle, che de' lamenti miei se' piena*, &c. he gives the most explicit intimation, that the grave of LAURA was in that very place, and amidst the same scenes where he so often had enjoyed the happiness of her society :

*Valle,*

*Valle, che de' lamenti miei se' piena ;  
Fiume che spesso del mio pianger cresci ;  
Fere silvestre, vaghi augelli, e pesci,  
Che l' una e l' altra verde riva affrena ;*

*Aria de' miei sospir calda e serena ;  
Dolce sentier, che sì amaro riesci ;  
Colle, che mi piacesti, or mi rincresci,  
Ov' ancor per usanza amor mi mena ;*

*Ben riconosco in voi l' usate forme,  
Non, lassò ! in me ; che da sì lieta vita  
Son fatto albergo d'infinita doglia.*

*Quinci vedea 'l mio bene ; e per quest' orme  
Torno a veder, ond' al ciel nuda è gita  
Lasciando in terra la sua bella spoglia.* Son. 32. Part. 2.

Thou lonely vale, where in the fleeting years  
Of tender youth I breath'd my am'rous pain ;  
Thou brook, whose silver stream receiv'd my tears,  
Thy murmurs joining to my sorrowing strain ;  
I come, to visit all my former haunts again !

O green-clad hills, familiar to my sight !  
O well-known paths, where oft I wont to rove,  
Musing the tender accents of my love !  
Long use, and sad remembrance now invite,  
Again to view the scenes which once could give delight !

Yes, ye are still the same !—though here I meet  
No more that angel form, which beauty shed  
On universal nature ! Her dear feet  
Oft trod your paths :—here rests in hallow'd earth her head !

IN one of his Latin eclogues, in which the poet celebrates  
LAURA under the fictitious name of GALATEA, three nymphs,  
NIOBE, FUSCA and FULGIDA, are introduced in conversation,  
and one of them asks the others to point out to her the place  
where

where GALATEA was buried ; which FULGIDA does in these words :

*Carpe iter, qua nodosis impexa capistris  
Colla boum, crebrasque canum sub limine parvo  
Videris excubias, gilvosque ad claustra molossos :  
Hic locus tua damna tegit ; jamque aspice contra,  
Hic GALATEA sita est.——*

“ Take your way yonder, where you will see the oxen yoked  
“ by the neck, and the watch-dogs guarding the entry of a small  
“ house. That hides from your sight what you are in search  
“ of ; for on the other side of it is the burial-place of GALA-  
“ TEA,” &c.

It is evident that the passages above quoted from the writings of the poet himself, ascertaining both the place of LAURA's birth and of her burial to have been in the country, in some small village or villa in the neighbourhood of the hills, and of the source of the Sorga, stand in direct contradiction to the manuscript note on *Virgil*\*, and to the sonnet said to have been found in the grave

\* THE note on *Virgil*, when contrasted with many passages of the poet's writings, exhibits likewise other intrinsic evidences of forgery. This note bears positive testimony, that PETRARCH saw LAURA for the first time at matin-prayers in the church of St Claire at Avignon. But, from many passages of the poet's writings, it appears, that his first interview with LAURA was in a solitary walk in the fields. Thus, in the 8th *Ballata*, Part. 1. (*Nova angeleta*):

*Nova angeletta sovra l' ale accorta  
Scese dal cielo in su la fresca riva,  
Là 'nd io passava sol per mio destino :  
Poi che senza compagna e senza scorta  
Mi vide ; un laccio che di seta ordiva  
Tese fra l' erba, ond' è verde 'l cammino :  
Allor fui preso. ——*

So,

grave of the church of the Cordeliers. The question, then, is, To which of the opposing proofs are we to give our faith? To this question we shall certainly not long hesitate to give an answer, when we consider, that the one class of proofs admits of no suspicion of fabrication or imposture; while the other is extremely suspicious, and may be altogether a forgery. It is impossible to suppose, that numberless passages, interspersed through the works of PETRARCH, indicating the place of LAURA'S birth and death, all naturally connected with the subject treated of, though some furnishing their evidence not in positive terms, but only by inference, should every one of them be fabricated; and that the forger should have been able

*Una candida cerva sopra l'erba  
Verde, m' apparve con duo corna d'oro,  
Frà due rivièrè a l'ombra d'un alloro.*

And yet more clearly, in the 3d of his Latin eclogues:

*DAPHNE, ego te solam deserto in littore primum  
Aspexi; dubius Dominæ Deamne viderem.*

The evidence arising from these passages, that PETRARCH'S first interview with his mistress was not in a church, but in the fields, is thus controverted by the Abbé de S A D E. Je suis persuadé que ces allégories qui présentent des images riantes de la campagne doivent être entendues des dehors d'Avignon, où des charmes du printemps, qui est la saison, dans laquelle PETRARCH vit LAURE pour la première fois. Convenoit-il qu'il parlât de l'église de S<sup>te</sup> CLAIRE en rapellant la première époque de son amour? Un poète qui parle de ces choses-là, est bien-aïlé d'égayer la scène; il n'ira pas la placer dans une église: et sur une chose si peu importante en elle-même, il ne doit pas s'attacher à la vérité historique; il se permet toutes les fictions qui peuvent rendre ses vers agréables." (Notes, vol. i. p. 57.). If this mode of reasoning is to be admitted, it is equally effectual against those passages which the Abbé has brought in support of his own hypothesis, as those which militate against it; and tends, indeed, to invalidate the whole evidence brought from the poems of PETRARCH in proof of any part of his history.

to insert all these fabricated passages in every one of the manuscripts of the author's works which are to be found in Europe. Nor even, should we allow this strange undertaking to have been practicable, is it possible to figure a motive capable of inducing to the attempt? For, what interest had any man to shew, that the LAURA of PETRARCH was born and died in an obscure residence in the country, while nothing more was proved to ascertain her origin or connections? But this reasoning will not apply to those proofs which are brought to shew that LAURA was born and died at Avignon. The manuscript note on *Virgil*, and the sonnet said to have been found in the grave, stand evidently in a very different predicament. Here the forgery was easy; the motive to it strong and alluring. At the distance of two hundred years from the death of PETRARCH, it was no difficult matter for the possessor of this manuscript of *Virgil*, which is said to exhibit a great number of notes on its margin, in the genuine handwriting of the poet, to have fabricated one additional note in imitation of that handwriting, of which he had before him so many specimens; nor did it require a great measure of ingenuity to compose a single sonnet, written in the person of PETRARCH, and that too perhaps of equal merit with many of his genuine compositions. Of both forgeries the motive was probably the same, which has instigated the Abbé de SADE to the compilation of his elaborate work, the desire of vindicating to this house the relation to so celebrated a personage as the LAURA of PETRARCH: though, as we shall presently see, the Abbé's hypothesis goes beyond the pretensions of his ancestors, and much further than the evidence of this note and sonnet can conduct him; even supposing both were of the most certain authenticity.

WE have already remarked, that the purpose of the search into the graves in the church of the Cordeliers, was to ascertain the

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the truth of that report, which gave the LAURA of PETRARCH to the house of SADE. Two or three persons, zealous in this research, unite their endeavours for that purpose, and their investigation is naturally directed to the family burial-place, which is a chapel of that church termed *Capella della Croce*. Among other monuments in this chapel a gravestone is discovered, which bears no inscription; and, on opening the grave, some bones are found, and a casket, containing a medal and a parchment. The medal has on it the figure of a very little woman. Here, say our antiquaries, is, in all probability, the figure of LAURA\*. Around the figure are four letters, M. L. M. I. This, says one of them, (MAURICE de SEVES), is undoubtedly to be interpreted, *Madonna LAURA morta jace*; though it is confessed that monumental inscriptions and the legends of medals are rarely found in the vernacular tongue; and though M. L. M. I. might have been read twenty different ways, with as much plausibility as in the way that MAURICE chooses to read it. The point, then, being already settled that this must be the grave of LAURA, the parchment, to be sure, must contribute its relative evidence to the same effect. This parchment, however, is found at first to be utterly illegible: a fact not at all surprising. The wonder is, that the parchment itself should have been discoverable: for, parchment being an animal substance, must, one should naturally suppose, have gone into total dissolution, in much less time than two hundred years, when inclosed in the same coffin with a putrefying carcase, from the juices of which a leaden box, shut only with a brass wire, would be a very insufficient protection. But the parchment is said to have been actually found. It could not, however, be read without the aid

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of

\* WE do not recollect any passage of the writings of PETRARCH which marks that LAURA was of small stature: nor is it easy to conceive how the medal, representing only a single figure, could accurately determine the size or stature of the person represented.

of the strongest light. It therefore was not read in the little dark chapel of the church \*. MAURICE de SEVES, in all probability, puts it in his pocket, and takes it home for his deliberate examination. He had already given his opinion on the legend of the medal, which ascertained LAURA's relation to the grave. His credit, therefore, as an antiquary, is at stake to support this opinion, by the further evidence of the rotten parchment; and next day he produces a fairly written sonnet, which he declares to have copied from the original, of which, with much pains, he had at length been able to decypher the characters. The original is never afterwards heard of. A copy, written likewise on parchment, is preserved at this day among the archives of the family of SADE, which they pretend to be the original, found in the grave, and regard accordingly as a most precious document. But, though mutilated a little to give it the air of antiquity, as its writing is extremely distinct, and may be easily read, that circumstance of itself demonstrates, that it cannot be the parchment which, even two hundred years ago, was not without the utmost difficulty decyphered by MAURICE de SEVES, with the aid of a very strong light.

THE sonnet or epitaph itself is generally esteemed a poor composition; and the Italian authors are indignant that it should ever have been suspected to have come from the hand of PETRARCH: but PETRARCH's compositions are not all of transcendent merit; and its intrinsic character is not, perhaps, of itself, sufficient to bring its authenticity into doubt. The medal and the lead box are said to have been seen about the middle of the present century, in the possession of the monks of the Convent of St CLAIRE; but there they are no longer; and the

Abbé

\* "SACELLUM, in quo illud (sepulchrum) videtur, obscurum est. Sinistra ingressus habet altare muro adstructum, ante quod, sub grandi saxo, sine omni ornatu et inscriptione, LAURA cubat." PHIL. THOMASINI, *PETR. Rediviv.* p. III.

Abbé de SADE mentions a shrewd conjecture of the Superior of the Convent, "that they had been sold to some *Seigneur Anglois*." Monks are very dexterous in the fabrication of all sorts of relics, and it was no bad policy of the *Fratres Minores*, to attract the notice of strangers to their convent, by the exhibition of a medal and a lead box, the evidences of a curious and disputed point of history: nor is it at all improbable, that afterwards a knavish friar, more studious of his own interest than that of his convent, might have purloined those precious relics, and got his own price for them.

SUCH is the history of those celebrated documents, which are said to prove the place of LAURA's birth and death to have been Avignon, and to ascertain that her grave was in the chapel of the church of the Cordeliers in that city, and in the burial-place of the family of SADE. I have stated fairly the whole of this evidence, which rests entirely on the manuscript note on *Virgil* and this miserable sonnet; and I leave that evidence to be balanced by those numberless passages of the poet's undoubted and authentic writings, which most unequivocally assert, that LAURA was neither born, nor died, nor was buried, at Avignon; but that she was born in a small village or country residence in the neighbourhood of Vaucluse, where she passed her life in tranquil and humble retirement\*; that she died there, and was buried in the same place. If the proofs on both sides

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\* *In nobil sangue vita humile e queta,* Son. 180. Part. 1.

*Non la conobbe il mondo mentre l' hebbe:*  
*Conobbil' io ch' a pianger qui rimasi.* Son. 67. Part. 2.

With what propriety or consistence with truth could the poet have thus expressed himself of LAURA de NOVES, the wife of a person of high rank, and who had passed the whole of her life in all the gaiety and splendour of the court of Avignon? Still less would the pious PETRARCH have borrowed a Scripture expression, addressed to the Saviour of the world, (St JOHN's *Gospel*, chap. xvii. v. 25.) and applied it, falsely too, to the object of an adulterous passion.

are impartially weighed, I am much deceived, indeed, if any person competent to judge of this species of evidence, will hesitate to decide that the latter is infinitely preponderating.

THE arguments by which the Abbé de SADE has endeavoured to invalidate the positive evidence contained in the writings of PETRARCH, that LAURA was born, lived and died in an obscure retreat in the country, are not undeserving of attention; as they are strong examples of that laboured sophistry, which is usually employed when one endeavours to support a weak or false hypothesis.

AVIGNON, says the Abbé, though a city of some celebrity, was far inferior in splendour to many of the Italian cities; and the mind of PETRARCH, being filled with the idea of their magnificence, might not unnaturally have termed the former *picciol borgo*. Unfortunately, PETRARCH at this time knew nothing of the splendid cities of Italy, but by description. He was born at *Arezzo*, an inconsiderable town in the Florentine territory; and he was no more than seven years of age, when he, together with his whole family, removed to Avignon. This city, therefore, was at any rate the most splendid he had ever seen; unless perhaps he had had a transient glimpse of Florence on his journey. It could, indeed, be only a distant prospect; for his father was then in a state of banishment from that capital, and durst not enter it. We may judge, then, with what propriety PETRARCH could have given the epithet of *picciol borgo* to Avignon, then the seat of the Papal residence, the perpetual resort of the most splendid embassies from all the sovereigns of Europe; and rendered illustrious, as the Abbé de SADE himself informs us, by frequent visits of the most celebrated princes, many of whom had actually built palaces there\*; a city, too, where, as

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\* "LES Princes, non contents d'envoyer des Ambassadeurs au Pape dans les moindres occasions, ne dédaignoient pas d'aller souvent en personne à Avignon, traiter avec

the same author acknowledges, the vast concourse of strangers had necessarily introduced every species of voluptuousness and debauchery \*.

BUT supposing for a moment that Avignon, thus populous, splendid and luxurious, really seemed a *picciol borgo* in the eyes of PETRARCH, whose mind, as the Abbé says, was filled with the great idea of the cities of Italy, (what we have indeed shewn to be only an *idea*, as he had never seen them), we cannot allow the same false estimate in the mind of LAURA, with respect to the most considerable city which, for certain, *she* had ever seen. If she had been born at Avignon, it certainly must have merited in her eyes a very different estimation from that of an *umil terreno*, which she was ashamed to avow as the place of her nativity. Yet such, we have seen, the poet describes to have been her estimation of the place of her birth; the only circumstance, she says, respecting her origin, of which she had reason to be ashamed †.

BUT the *picciol borgo* and the *umil terreno*, says the Abbé, was not properly the city of Avignon itself; it was only a suburb, a *fauxbourg* of that city, and thus might well have merited those epithets.

avec lui des grands intérêts de leurs états. Le Roi de Majorque et le Dauphin de Viennois y avoient même des demeures fixes.—Le palais du Roi de Naples étoit où l'on voit à-présent le monastère de S<sup>te</sup> URSULE, qu'on appelle *les Royales*, parcequ'elles habitent le palais d'un Roi. Un grand concours d'étrangers dans une ville, y entraîne nécessairement la licence et la débauche," &c. *Mem. de PET.* tom. i. p. 68.

\* OF the very great populousness of this city at that time, we may form a general idea from a remarkable and melancholy proof. The pestilence which ravaged Italy and the south of France in the year 1348, and of which Boccaccio has given, in the introduction to his *Decamerone*, a most eloquent and impressive description, cut off, in the space of three months, a hundred and twenty thousand of the inhabitants of Avignon. *Hist. Pissol. Mem. de PETR.* tom. ii. p. 456.

† SEE *suprà*, p. 132.

epithets. To this we shall answer by a single question, which has been well put by M. de la BASTIE to those who contend for LAURA'S being born at Avignon: "Que dirions-nous d'un poète, qui adressant la parole à quelqu'un qui seroit né dans le fauxbourg St<sup>e</sup> Germain, croiroit devoir le plaindre de ce qu'il n'est pas venu au monde dans une ville considérable? Ceux qui naissent dans ce fauxbourg, sont-ils moins nés à Paris?"

BUT, continues the Abbé, we find, in a fragment which is printed at the end of all the editions of the works of PETRARCH, the following expression:

*Dove Sorga e Durenza in maggior vaso  
Congiungon le lor chiare e torbide acque;—  
Ivi, ond' a gli occhi miei el bel lume nacque.—*

"That beautiful luminary was born where the *Sorga* and the *Durance* unite their clear and turbid streams in a larger channel." Can any thing, says he, mark more precisely the situation of Avignon?

THE answer is, *first*, The authenticity of this fragment is not admitted. It is not found in the best manuscripts of the works of PETRARCH, nor have VELUTELLO, GESUALDO, or BEMBO, given it a place in their editions. In the splendid edition printed at Venice in 1756, it is found in an appendix, which the editor entitles, "Giunta d'alcune compositione del PETRARCA che si dicono da lui rifiutate." It is therefore a document of no authority whatever. But, *secondly*, Even supposing it genuine, the description there given does by no means mark precisely the situation of Avignon. The *Sorga* and the *Durance* do not join their streams at Avignon. The *Sorga* falls into the Rhône five miles above Avignon, and the *Durance* six miles below that city. These rivers, therefore, though they unite their streams by both  
falling

falling into the Rhône, do not mark out Avignon as the point of junction, but rather mark the boundary of one side of a district termed the *Venaissin*, which comprehends the country for several miles adjacent to Avignon; and therefore, the description is equally indicative of *Vaucluse* and of *Cabrieres*, as it is of Avignon.

BUT this passage, supposing it genuine, will find its best explanation by a similar one, which occurs in the 10th of the poet's Latin eclogues, entitled *LAUREA occidens*; in which he bewails the death of his mistress under his favourite allusion of a *Laurel*:

————— *Fuit alta remotis,*  
*Silva locis qua se diversis montibus ætæ*  
*Sorga nitens Rhodano, pallensque Ruentia miscent.*  
*Hic mihi, quo fueram Tusco translatus ab Arno,*  
*Sic hominum res fata rotant, fuit aridulum rus,*  
*Dum colui indignè, atque operi successit egestas. —*  
*Verum inter scopulos nodosaque robora quercus,*  
*Creverat ad ripam fluvii pulcherrima Laurus :*  
*Huc raptor. —*

*Has ego delicias et opes, hæc regna putavi. —*

“ In a remote quarter of that country where the Sorga and Du-  
 “ rance unite their streams, was a thick forest, where, after I  
 “ was removed from the Tuscan vale of *Arno*, I possessed a little  
 “ barren country seat,—Here, amidst the rocks and thickets of  
 “ oak, near the borders of the stream, grew a most beautiful  
 “ *Laurel*. This favourite object engrossed all my care. In this  
 “ spot was my kingdom, and here I found my supreme delight.”  
 Nothing certainly can more accurately picture the scenery of  
*Vaucluse* and its vicinity, where the poet feigns his beautiful  
*Laurel* to have sprung, amidst the rocks and thickets.

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IN the 13th Canzone (*Se 'l pensier*), the poet addresses himself to a beautiful stream, on the borders of which his mistress was wont frequently to walk. The commentators, in endeavouring to identify the scenery to which this poem refers, have, with great appearance of probability, supposed this rivulet to be the *Coulon*, which runs near to Cabrieres, where LAURA is believed to have dwelt, and conclude it to have been the same rivulet in which he had once surpris'd his mistress bathing, quite naked, an incident to which he alludes in the first *Canzone*. But this supposition, contradicting his theory, appears to the Abbé de SADE quite unnatural and absurd. He finds a basin or pond in a garden close by the walls of Avignon, which corresponds to a miracle with every thing here alluded to. As to the rivulet of Coulon, says he, it is no less than a *mile and a half* distant from Cabrieres, a circumstance which puts its pretensions out of the question; as this would have been rather too long a walk for a lady, *promenade un peu forte pour une dame*; and PETRARCH himself must have cross'd a steep hill, and walked at least *four miles and a half*, before he could have seen her there.

IN a similar strain of weak and inconclusive reasoning, this author attempts to invalidate the evidence of the sonnet with which PETRARCH accompanies his present of the birds, caught at the foot of those hills where lay the birthplace of LAURA. Part of the city of Avignon, says the Abbé, is situated on a rocky eminence; and although the foot of that rock is now all built over, and comprehended within the precincts of the city, yet, in those days it *might* have been open ground, and PETRARCH *might* there have amus'd himself in fowling, and have caught the birds in question.

So likewise in the 184th sonnet (*Il cantar nuovo*), where the poet describes the pleasures of the morning in the country, the valleys resounding with the sweet song of the birds, and the  
murmuring



murmuring of the clear rivulets\*, and tells, that rising early to hail the morning rays, he had the fortune to see two suns rise at the same moment; the sun that marks the vicissitude of day and night, and LAURA, who at that instant eclipsed his radiance by her own: the Abbé, with his usual facility of perversion, cites this sonnet as a proof that PETRARCH used frequently to walk the streets of Avignon before daybreak, to have the pleasure of seeing his mistress open her windows.

BUT the most amusing instance of this sophistical perversion of an author's clearest expressions, is to be found in the construction which is attempted to be put on the passage above quoted from the Latin eclogue of GALATEA †.

*Carpe iter, qua nodosis impexa capistris, &c.*

“Take your way yonder, where you will see the oxen yoked by the neck, and the watch-dogs guarding the entry of a small house.—On the other side of it is the burial-place of GALATEA.” This has been ever reckoned as one of the most decisive testimonies, that the burial-place of LAURA was not in the city of Avignon, but in the country. It was necessary, therefore, to take off the force of this strong piece of evidence. It is admitted,

\* *Il cantar nuovo, e 'l pianger de gli augelli.  
In su'l dì fanno risentir le valli,  
E 'l mormorar de' liquidi cristalli  
Giù per lucidi freschi rivi e snelli.—  
Così mi sveglia a salutar l'aurora,  
E 'l sol, ch'è seco, e più l'altro, ond'io fui  
Ne prim'anni abbagliato, e sono ancora:  
I' gli ho veduti alcun giorno ambedui  
Levarsi insieme, e 'n un punto e 'n un hora  
Quel far le stelle, e questo sparir lui.*

Son. 183. Part. 1.

† See *supra*, p. 138.

admitted, on all hands, that LAURA is figured under the name of GALATEA : but the yoked oxen, and the watch-dogs guarding the entry of a small house, how can they possibly be applied to a church in the city of Avignon, or indeed to any thing else than a farm-house in the country? Nothing is more clear, say the advocates for this hypothesis; the *oxen* yoked by the neck are the *friars* of the convent, the *fratres minores*, *qui ferunt jugum obedientie, laboris et religionis*; the *dogs* guarding the door are the same *friars*, (both *dogs* and *oxen*), who may well be termed *dogs*, because they *bark* so much in their sermons; *frequentes vigiliis canum, prædicantium scilicet et latrantium*.

I PRESUME I have given a sufficient specimen of the Abbé de SADE's ingenuity in accommodating and bending to his own purpose those passages of the poet's writings which are in the most direct contradiction to his hypothesis; nor would an unprejudiced critic desire a more satisfying proof of its futility, than the miserable shifts to which its defenders are put in order to support it. This hypothesis, however, must yet be more thoroughly canvassed; and, for that purpose, I begin with a brief detail of its whole particulars.

THE Abbé de SADE endeavours to prove, that the mistress of PETRARCH was LAURA de NOVES, the daughter of AUDIBERT de NOVES, a gentleman of noble birth in Provence; that she was born at Avignon, in 1307 or 1308; that she was married, in 1325, to HUGH de SADE, the representative of a very ancient and honourable family in the territory of Avignon, to whom she bore eleven children; that she died at Avignon, in the year 1348, and was interred in the burial-place of the house of SADE, in the Church of the Cordeliers in that city. The Abbé, who is himself a younger son of that family, has proved the whole of those leading circumstances, in the history of his *progenitrix* LAURA de NOVES, by authentic documents in the archives of the house of SADE; and he has particularly established the cer-

tainty of these facts, "that this LAURA, *the wife of HUGH de SADE*, was born, died, and was buried at Avignon." If, therefore, it is equally demonstrable that the LAURA of PETRARCH was neither born, died, nor was buried at Avignon, it is plain she was a different person from LAURA de NOVES. The evidence of these latter facts we have already seen. It is clear, positive, and unambiguous. It might, therefore, be sufficient to rest the matter here without farther argument; for an hypothesis must fall of itself, when the main props on which it is built are demolished. But I am tempted to go a great deal farther: and, allowing that the question as to the place of LAURA's birth and death were still a matter of doubt; nay, even admitting, for the sake of argument, that the LAURA of PETRARCH had, according to the tradition current at Avignon, been connected, in some way or another, with the house of SADE, and on that account had been interred in the burial-place of that family; supposing, in short, the authenticity both of the note on *Virgil*, and the whole story of the grave, the medal and the parchment; I shall now proceed to shew, and, as I trust, to prove to absolute conviction, that this LAURA, connected as we shall suppose her with the house of SADE, and buried in their family-vault, *was never married.*

AND here, I must previously remark, that the report which, it is allowed, was current at Avignon, that LAURA belonged to the family of SADE, was not that she was *connected by marriage* with that family, but that she was herself a descendant of that house. The opinion that she was connected with it by marriage, was never entertained by any one of the family, till it found its origin in the whimsical vanity of this author of the *Mémoires*, the Abbé de SADE, who has laboured, in this voluminous work, to convince the world, that he himself is actually sprung from the body of the illustrious LAURA. That she was *a daughter* of the house of SADE, had been a tradition, though, as we have

seen, a very ill founded one, for some centuries. "The opinion regarding LAURA," says VELUTELLO, "which was current at Avignon, may have, in a great measure, arisen from one GABRIEL de SADE, an old nobleman of that city, with whom I happened to converse a great deal at two different times, when I was at Avignon. He set forth, that he was descended from one HUGH de SADE, a brother of JOHN, who was *the father of Madonna LAURA*, whom he understood to have been the same who was celebrated by PETRARCH; and he told me, that this JOHN de SADE had some possessions at *Gravesons*, about two leagues from Avignon, where he lived always in summer, but passed the winter at Avignon; and that this LAURA, the daughter of JOHN de SADE, was, after her death, interred in the family burial-place, in the Church of the Cordeliers. But," continues VELUTELLO, "what convinced me that this old man's story was altogether a fiction, was, that being asked at what time this LAURA lived, he said, that by a certain testament which he had seen, she must have been a grown woman between the years 1360 and 1370: Now we are certain," adds VELUTELLO "that PETRARCH's LAURA died in the year 1348."

I QUOTE this passage for two reasons; *first*, To shew that VELUTELLO, who lived within 150 years of the time of PETRARCH, who had been at the utmost pains to investigate every trace of the history of LAURA, (a subject which he has treated in a separate dissertation), and who had himself conversed with this old gentleman of the family of SADE, from whom he supposes the report of LAURA's relation to that family to have actually originated, did not himself give credit to the story, but conjectured it to be a mere fiction, and the offspring of the old man's vanity; and I mention it, To shew, *secondly*, that this report, thus traced nearly to its origin, did not make LAURA a *married woman*, or connected with the house of SADE by marriage, but

a *daughter* of one of that family, namely of JOHN de SADE. At this time, therefore, when it may be presumed the relation in which this celebrated person stood to their house, if there was any truth in it at all, must have been better known than it is now, we find there is not the smallest idea of that hypothesis, which the present representatives of this family are so anxious to establish, namely, that she was connected with them by marriage. Instead of being the wife of a M<sup>r</sup> de SADE, and the mother of a numerous progeny, we find, on going back two centuries, that the family themselves believe her to have been a *daughter* of one of that house; nor is it discovered till the present age, which is above 400 years after this lady's death, that she was the *wife* of their ancestor, the mother of all the surviving branches of that house; and consequently, that from her illustrious blood is sprung the whole race, now existing, of that family, and among the rest the ingenious Abbé, the author of this important discovery, which it has been the labour of his life to prove and authenticate.

ONE should have naturally imagined, that this gentleman, so proud of his ancestry, might have remained content with that portion of renown which appears to have satisfied the vanity of his forefathers, the attributing to their house the honour of having produced this illustrious lady. And indeed it is not easy to conceive how, in any just balance of moral estimation, the one species of adscititious merit should outweigh the other. On the contrary, a rigid moralist would infallibly decide, that there was more real honour that accrued to a family from having produced the pure, the chaste, the coy, the maiden LAURA, the model of female dignity and propriety, the object of an ardent, but virtuous affection to the most illustrious character of the age; than from having acquired by marriage, a connection with a lady, who, whatever were her personal charms, had no title to the praise of exalted virtue, or of true female dignity; who,

who, while joined in wedlock to a respectable husband, and the mother of eleven children, continued, for above twenty years, to put in practice every artifice of a finished coquette, to ensnare the affections, and keep alive the passions, of a gallant, whose attachment, from the celebrity of his name, was flattering to her vanity.

I MUST indeed acknowledge, that these notions are drawn from a system of morals, with which the Abbé de SADE and most of his countrymen are but very little acquainted. I know that, in the opinion of most Frenchmen, a handsome married woman derogates not in the slightest degree from the rules either of virtue or of strict propriety, while she amuses herself with the gallant attentions of all the young men of her acquaintance; and the most intimate reciprocation of tender sentiments, while it is only an affair of the heart, is termed *une belle passion* \*. This is precisely what the Abbé de SADE supposes to have been the connection of PETRARCH and of LAURA. PETRARCH besieged her with ardent and importunate solicitations, which had for their object the ordinary rewards of a lover. She never actually dishonoured her husband's bed; but she made no scruple to avow to her lover that her heart was sensible to his flame; though at times she found it necessary to feign a rigour and coldness of demeanour in order the better to keep alive the ardour of his passion. "Par ce petit manège," says the Abbé, "cette alternative de faveurs et de rigueurs bien ménagée, une femme tendre et sage amuse, pendant vingt et un ans, le plus grand poëte de son siècle, sans faire la moindre breche à son honneur."

\* Our author has even termed this amour, *une passion honnête*, (an honourable passion). Thus, in speaking of Avignon, he says: "Une ville qui fait gloire de l'avoir élevé dans son sein, et d'avoir été le théâtre d'une passion honnête, qui lui a inspiré de si beaux vers." *Mém. de PET.* tom. i. p. 29. And the same expression occurs, tom. i. p. 111. where the Abbé proposes this passion of the poet for the wife of another man, "as a model for all tender and virtuous hearts."

"honneur." *Mém. pour la Vie de PET. Préface aux François, &c.* And in the following passages he describes this *petit manège* more particularly:

"LAURE, qui ne vouloit ni se donner à lui, ni le perdre, n'eut pas plutôt aperçu les nouveaux efforts qu'il faisoit pour briser ses fers, qu'elle mit en usage pour l'y retenir les petites ruses qui lui avoient jusqu'alors si bien réussi: air moins sévère, regards plus doux, petits mots en passant," &c. *Mém. vol. ii. p. 311.*—"LAURE ne pouvoit se résoudre à perdre un amant de cette trempe, qui l'aimoit depuis onze ans, et qui faisoit de si beaux vers pour elle: le rencontrant un jour dans les rues d'Avignon, elle jeta sur lui un de ces regards qui savoient si bien le ramener." *Ibid, p. 383.*—"Ces petites ruses d'une coquetterie innocente, que LAURE savoit si bien mettre en œuvre, eurent bientôt produit leur effet ordinaire." *Ibid. p. 386.*

FROM these passages we may judge, what idea this Reverend Abbé has formed to himself of the character of a respectable married woman, and the mother of a family; *une femme tendre et sage*, who has the art to keep alive, by all the tricks of a coquette, the passions of her gallant for no less than twenty-one years; and all this *sans faire la moindre breche à son bonneur*. Observe too, that, for the better part of this period of fascination, the lady herself could owe little of her power to her personal charms; for, if our author's hypothesis be true, and LAURA de NOVES, the wife of HUGH de SADE, was the mistress of PETRARCH, this lady must have been a grown woman before her marriage in the year 1325; she had been married three years before PETRARCH first saw her; she bore eleven children in the period of their acquaintance; and she died when she was about forty years of age, *with all the symptoms of an exhausted constitution*. Yet all the while, this hackneyed and antiquated coquette, regardless of the character of a wife and a mother, is practising

her *petit manège* of alternate favours and rigours to turn the head of an infatuated *enamorado*, whose passion was in itself an affront to virtue and morality, and amuse him for a lifetime with the expectation of favours which she is determined never to grant. Such, in the system of the Abbé de SADE, is the accomplished LAURA, and such the respectable and virtuous PETRARCH. How absurd, how disgusting, how contemptible the one: how weak, how culpable, how dishonourable the other!

BUT let us now examine the particulars of that evidence on which this author has built an hypothesis, so degrading to those characters whom the world has hitherto united to venerate and admire.

THE Abbé de SADE has, in a note at the end of the first volume of his work, given a short abstract of the arguments which he has drawn from the works of the poet himself, to shew that the LAURA of PETRARCH was a married woman. I shall take them in the order in which they are given\*.

“ALMOST all the world,” says he, “has believed that LAURA was unmarried: *Presque tout le monde a cru que LAURE étoit fille.*” VELUTELLO lays it down as a proposition absolutely certain, “*Per cosa certa habbiamo da tenere che non fosse mai maritata.*” Nevertheless,” says he “it is an undoubted truth that she was a married woman. PETRARCH himself expresses it in such a manner as to put it beyond all question.

“*Imò*, In speaking of LAURA, he terms her, in his Latin works, always *mulier* and *fœmina*, and never *virgo* or *puella*; and in his Italian works, always *madonna* or *donna*, and never

\* In a small pamphlet, entitled “*An Essay on the Life and Character of PETRARCH*,” written by the author of these Remarks, and printed in 1784, a brief summary is given of the Abbé de SADE’s arguments proving LAURA to be a married woman, to which the answers are in substance much the same with what the reader will find here, though they are now given in a more ample form, and strengthened by additional matter of proof from the writings of PETRARCH.



“ *vergine, donzella*, appellations always applied to unmarried women, and which sound so peculiarly graceful in both languages.

“ 2dò, IN sonnets 10. 162. &c. the poet speaks of the dress of LAURA, of the garlands she wore on her head, of the jewels and pearls with which she braided her hair; and, in sonnets 151. and 158., he mentions the magnificence of her garments. Now, in the age of PETRARCH, the young unmarried women wore neither garlands, nor pearls, nor jewels; they were dressed in a very simple manner, and appeared very little in public.

“ 3tò, IN sonnets 162. and 185. PETRARCH complains, that jealousy often deprived him of the pleasure of seeing LAURA. Some commentators have indeed supposed this to be meant of the jealousy of her parents, watchful over the honour of their daughter: but this is a forced and unnatural construction; for the term jealousy, *gelosia*, has never been applied to the watchful care of parents for the honour of their children.

“ 4tò, PETRARCH, with the intention of celebrating the victory which LAURA had gained over love, composed, after her death, a poem, entitled The Triumph of Chastity, *Trionfo della Castità*. If LAURA had been a virgin, it is clear that he would have entitled his poem *Trionfo della Virginità*. In this poem, too, he would have given LAURA virgins for her attendants, and not married women. Now, those who follow LAURA to the Temple of Chastity, are LUCRETIA, PENELOPE, JUDITH, DIDO, the Greek HIPPO, HERSILIA, &c. all married women. The single exception is a Vestal Virgin.”

THESE, the author of the *Mémoires* acknowledges, after all, amount to nothing more than strong conjectures. Now follows, says he, something more conclusive.

“ 5tò, PETRARCH, in one of his dialogues with St AUGUSTINE, in speaking of LAURA, says, that her body was exhausted by

" frequent childbearing; *corpus ejus crebris partibus exhaustum*.  
 " It is true, that the word *partibus* is thus abbreviated in the  
 " manuscript *ptbs*; and, as it was formerly the current opinion  
 " that LAURA was unmarried, those who had the charge of  
 " printing the Latin works of the poet, have thought proper to  
 " interpret the abbreviation *perturbationibus*, and to print it so  
 " in all the editions of those works: But it ought certainly to  
 " be read *partibus*, for these good reasons: *first*, That the epithet  
 " *crebris* means a repetition of acts, and therefore applies better  
 " to acts of childbearing than to passions. If the author had  
 " meant the latter, he would have coupled the noun with *multis*,  
 " instead of *crebris*. But what passions can we suppose to have  
 " exhausted the constitution of the most prudent and modest of  
 " women, who led a life so simple and so uniform? In the *next*  
 " place, Messrs CAPERONNIER, BOUDOT and BEJOT, of the  
 " King's Library, who must be allowed to be good judges of  
 " the abbreviations that occur in old manuscripts, have de-  
 " cided, that *partibus* is the proper reading."

SUCH is the whole of that evidence, drawn by the Abbé de  
 SADE from the works of the poet himself in support of this new  
 hypothesis, that LAURA was a married woman. On this evi-  
 dence, which, it will be allowed, is of itself extremely inconclu-  
 sive, I shall now make some remarks. I take the author's ar-  
 guments in the order in which they stand.

*Imò*, THE words *mulier*, *fœmina*, in Latin, and *donna*, *madonna*,  
 in Italian, are equally applicable to married and to unmarried  
 women. *Mulier* and *fœmina* mark the sex alone, without refe-  
 rence to the state or condition. ISIDORUS, in his *Origines*, l. xi.  
 c. 3. says: "*Dicitur igitur mulier secundum fœmineum sexum, non*  
 "*secundum corruptionem integritatis; nam EVA statim facta de*  
 "*latere viri, et nondum contacta a viro, Mulier appellata est, di-*  
 "*cente Scripturâ, Et format eam in mulierem.*" Thus too, in the  
 Roman law, where there is the utmost precision in the use of  
 terms,

terms, *mulier* and *fœmina* are indiscriminately applied to unmarried and to married women. In the 17th law of the *Codex de donat. ante nuptias*, the title bears; “*Donatio ante nuptias facta mulieri in minori ætate, non indiget insinuatione,*” &c. FABER, in his *Thesaurus*, observes, on the word *mulier*; “*Variè accipitur vox; aliquando enim communiter de sexu dicitur, et omnem ætatem ac conditionem ejus amplectitur; itaque et de puellis usurpatur,*” &c.

In the same manner, *donna* and *madonna* are used by the Italians, when speaking either of unmarried or of married women. Every Italian poet terms his mistress *donna* and *madonna*. Thus, ARIOSTO, in the beginning of the 35th canto of the *Orlando Furioso*, says,

*Che salira per me, Madonna, in cielo;*

and in his 1st elegy,

*Non è assai Madonna mesi e anni  
La fra speme e timor fin qui sospesa?*

Thus GUARINI, in the *Pastor Fido*, att. 1.

*La fede in cor di donna, se pur fede  
In donna alcuna (ch'io no'l fo) si trova, &c.*

and again,

*Bella donna e gentil, sollecitata  
Da numerosa stuol di degni amanti,  
Se d'un solo e contenta, e gli altri sprezza  
O non e donna, O se pur donna, e sciocca.*

Thus too, BOCCACCIO, in the Introduction to the *Decamerone*:

*Gratiose e nobili donne, meco pensando, &c.*

A thousand instances of the same kind might be given, to shew, that this criticism of the author of the *Mémoires* on the words *mulier*, *fœmina*, *donna* and *madonna*, has no solid foundation.

2dò, THE author of the *Mémoires*, when he says that, in the age of PETRARCH, the unmarried women were always simply dressed, and that the use of garlands, of pearls, and of jewels, was peculiar only to such as were married, assumes a fact of which there is no evidence. MURATORI, in his twenty-fifth dissertation on the dress of the middle ages, proves, that in the north of Italy, about the time of PETRARCH, the dress of the women was remarkably splendid; and he makes no distinction between the dress of the married, and of the unmarried women. He quotes a monk, GALVANEUS, who, inveighing against the luxury of the times, says: “Mulieres similiter in pejus omnia mutaverunt. Ipsæ namque stragulatibus vestibus, scopato gutture et collo redimitæ fibulis aureis, gyrovagantur. Sericis et interdum aureis indumentis vestiuntur. Crinibus crispatis more alienigenarum capite perstringuntur. Zonis aureis fupercinctæ, Amazones esse videntur.” MURAT. *Antiq. Ital.* tom. ii. p. 417.—If such was the splendid attire of the women in the north of Italy, the Court at Avignon importing thence both its manners and its fashions, would not, it is probable, be behind their models in dress, as in every other species of luxury. We have the authority of the Abbé de SADE himself for affirming, that, under the pontificate of CLEMENT VI., the dress of the women at the Court of Avignon was splendid and luxurious in the extreme, (*Mém de PET.* tom. ii. p. 92.); and that this affectation had reached even the lower classes, appears from a proclamation, which it was found necessary a short time afterwards to issue, prohibiting the use of gold, silver, ermines or silk, in apparel, to all women, unless the relations of the Pope, the wives and daughters of the Lord Mareschal and Lord Vicar, the

the Baroneſſes and the Ladies of Quality of the city \*. Now, from the evidence of this document, extending the exception to the *wives* and *daughters* of perſons of a certain rank, we are undoubtedly warranted to infer, that this ſplendour and luxury of dreſs of the ladies of Avignon, was common to both the unmarried and married women. We know for certain that LAURA was a woman of noble blood. Of this the poet informs us in numberleſs paſſages of his writings. Conſidering, therefore, her rank and condition, her dreſs will certainly appear to diſplay no extraordinary magnificence. In the 10th ſonnet, (*Se la mia vita*), the poet ſays no more than that his miſtreſs wore garlands and green clothes †. In the 162d ſonnet, (*L'aura ſerena*), he ſays, that her hair, which formerly ſhe wore looſe, was now braided and adorned with pearls and jewels ‡; a cuſtomy diſtinction of dreſs, as CASTELVETRO remarks, between young girls (*fanciulle*), and grown women. Now, if the remark of the Abbé de SADE be juſt, that this mode of adorning the hair was peculiar to married women, thoſe paſſages which indicate that LAURA adorned her hair in that manner will prove a great deal more than the Abbé intends they ſhould do: for they will demonſtrate that PETRARCH was acquainted with LAURA while

\* “ Quod nulla domina, ſeu mulier, cujuſcunque conditionis exiſtat, exceptis dominabus de parentela domini noſtræ Papæ, et uxorum ac filiarum dominorum Mareſchalli et Vicarii, et exceptis etiam dominabus baroniſſis et majoribus in civitate habitantibus, nunc et in futurum auſa ſit portare in aliqua rauba ſeu veſte, aliquem reverſum de ſubtus nec de ſupra, neque in martis variorum, erminiorum, ſeu quarumcunque aliarum pellium, ſeu rerum, ſive de auro, de argento, nec de ſerico.”  
*Præconiſationes anni 1372.*—*Mém. de PET.* tom. 2. p. 92.

† *E laſſar le ghirlande, e i verdi panni.*—

‡ *E le obiome lor avvolte in perle, e'n gemme,  
Alhora ſciolte, &c.*

while she was unmarried, as they prove, that he had formerly seen her while she wore no ornaments on her head, but appeared with her hair loose, unbraided, and quite unadorned, as the Abbé supposes to have been the fashion of the young unmarried women. Thus, in the 162d sonnet, above quoted, the poet says: "The serenity of the air, and the return of spring, bring  
 " to my remembrance the time when I first felt the power of  
 " love; when I first beheld that beautiful countenance, and saw  
 " those golden locks loosely waving in the wind, which are now  
 " braided and adorned with pearls and jewels \*." And so likewise, in the 69th sonnet, P. 1. describing the time when he first saw LAURA, he says: "Those golden tresses were then loosely  
 " scattered by the wind, which twisted them into a thousand  
 " beautiful ringlets:"

*Erano i capei d'oro al'aura sparsi,  
 Che 'n mille dolce nodi gli avvolgea.*

If, therefore, as the Abbé de SADE maintains, this braiding and adorning of the hair marked the distinction between the married and unmarried women in the age of PETRARCH, he must admit, on the evidence of those passages where her headdress is so described, that, at the time when the poet was at first acquainted

\* *L' aura serena, che fra verdi fronde  
 Mormorando à ferir nel volto viemme  
 Fammi risovvenir, quand' Amor diemme  
 Le prime piaghe, sì dolce e profonde;  
 E 'l bel viso veder, ch' altri m'asconde  
 Che sdegno e gelosia celato tiemme;  
 E le chiome hor avvolte in perle e'n gemme,  
 Albora sciolte, e sovra or terse bionde:  
 Le quali ella spargea sì dolcemente,  
 E raccogliea con sì leggiadri mani  
 Che ripensando ancor trema la mente.—*

quainted with LAURA, and became enamoured of her, as she wore her hair in loose ringlets, she was *unmarried*; a circumstance destructive of his whole hypothesis: for LAURA de NOVES was married to HUGH de SADE, as the Abbé has shewn from his own family documents, in 1325; and PETRARCH saw *his* LAURA for the first time in 1327. I shall leave the Abbé to extricate himself from this dilemma the best way he can: for my part I see no possible means of an escape.

BUT the author of the *Mémoires* has, on the subject of the dress of LAURA, either wilfully perverted, or most palpably misunderstood, his authorities. He has quoted sonnets 151. and 158. † in proof of that richness of apparel, which he argues to be characteristical of her being a married woman. The first of these sonnets is a comparison of the poet's mistress to the fabled phoenix, which is thus described by PLINY: "Auri fulgore  
" circa colla, cætera purpureus, cœruleam roseis caudam pennis  
" distinguuntibus." *Nat. Hist.* lib. 10. c. 2.—So PETRARCH, in this sonnet, describes his beautiful Phœnix; her lovely hair *artlessly* floating in ringlets about her neck, and thus forming a natural necklace of gold; her shoulders covered with a purple garment bordered with azure\*; thus, in every point resembling the famed Arabian bird. The former part of this description,

which

\* *Questa Fenice de l'aurata piuma  
Al suo bel collo candido, gentile  
Forma senz' arte un sì caro monile  
Ch' ogni cor addolcisce, e 'l mio consuma:  
Forma un diadema natural. —  
Purpurea vesta d'un cœruleo lembo  
Sparso di rose i belli boveri velu;  
Novo babito, e bellezza unica e sola.  
Fama nell' odorato, e ricco grembo  
D' Arabi monti lei ripone e cela;  
Che per lo nostro ciel si altera vola.*

† SONNETS 152. and 159. of the Venice edition, 1756.

which applies to the hair, is evidently characteristic of the utmost simplicity of female decoration, and therefore lends no aid to the Abbé's laboured argument. The latter part describes only the colour of the garments of LAURA, and might apply equally to a fine lady, and to a country maiden who wore a crimson cloak with a blue border. As to sonnet 158. which is quoted as proving that LAURA wore garments embroidered with gold and pearls, our author has fallen into a very gross mistake, in giving a literal meaning to what is entirely figurative. The poet is there speaking, not of the dress or garments, but of the personal qualifications and shining accomplishments of his mistress.

*Vedi quant' arte dora, e' mperla e' nnostra  
L' habito eletto.—*

This passage CASTELVETRO thus properly interprets: "*Abito* " in questo luogo significa *corpo*, che e come abito e vestimen- " to al' anima, il quale e ornato di maravigliose bellezze e ma- " niere, che egli significa, dicendo, che l'arte lo 'ndora e 'mperla, " e' nnostra, come si farebbe una veste." And such is the interpretation which all the commentators have put on this passage.

3<sup>th</sup>id, IN sonnet 162. \*, the poet complains, that jealousy had deprived him of the sight of LAURA; and in sonnet 185. † the female companions of LAURA make the same complaint: but in neither of the passages alluded to is there the smallest hint that the jealousy of a husband was here meant. Unless, therefore, the author of the *Mémoires* shall shew, that there can be no jealousy except in the breast of a person who is married, his argument concludes nothing. The word *gelosia*, in Italian, is no more limited in its signification than the English word *jealousy*; both

\* *L'aura serena.*

† *Liete, e pensose.*



both meaning the resentment of every species of rivalry. In the first of the sonnets above mentioned, it is most probable that the poet meant that LAURA's own jealousy had frequently deprived him of the happiness of seeing her \*. What gives the strongest support to this interpretation, is the association of disdain with jealousy:

*E 'l bel viso ch' altri m'asconde  
Che sdegno o gelosia celato tiemme.*

And we know, from some particulars of the life of PETRARCH, that LAURA had sufficient cause both of disdain and jealousy †. It is in this mortifying feature, of an otherwise most virtuous and exemplary character, that we are to seek for the true reason of those changes of deportment which LAURA manifested towards

\* ON this passage CASTELVETRO thus remarks: " Si potrebbe intendere di LAURA che, sdegnata col PETRARCA, gli nascondesse il viso: O, perche fosse innamorata di se stessa, avesse gelosia che il PETRARCA la vedesse:

*Se forse ogni suo gioia  
Nel suo bel viso è solo,  
E di tull' altro è scbiva.* Canz. 13.

Ma meglio è d'intendere de' parenti,

*Dogliose per sua dolce compagnia  
La qual ne toglie invidia e gelosia.*— Son. 185."

† We learn from his familiar letters, that his passion for LAURA had not restrained him from the indulgence of a meaner amour, with a woman of low manners and of a disagreeable temper; a passion, of course, in which the heart had no share; and that, in consequence of this connection, which was even of some years duration, and was a source to him of much disquiet, he had a natural son and daughter; of the former of whom we find frequent mention made in the course of those letters.

towards her lover, those alternate marks of favour and of cold reserve, and that tedious protraction of the final reward of a passion, unexampled in its ardour and duration.

IN the 185th sonnet, where the female companions of LAURA complain that envy or jealousy had deprived them of her company, the expression may be meant either of her own jealousy, as in the former instance, or more probably, in this place, of the jealousy of her parents. “*Restata in casa per invidia o gelosia de’ parenti*,” says CASTELVETRO: and the same author remarking that some have suspected from this passage that LAURA was a married woman, acutely observes, that the context plainly indicates that the expression will not admit of that construction: Her companions lament, that they are deprived of her company by that envy or jealousy which repines at the happiness of another, as if it were its own misfortune:

*Dogliose per sua dolce compagnia  
La qual ne toglie invidia e gelosia  
Che d'altrui ben, quasi suo mal, si dole.*

“The man who is truly jealous,” he well observes, “cannot be said to repine at the happiness of another, *as if it were* his own misfortune; for, in reality, *it is* his own misfortune.”

4<sup>to</sup>, THE author of the *Mémoires* is equally ill-founded in the argument he endeavours to draw from the title of the *Trionfo della Castità*, as in most of his other critical remarks. *Castità* in Italian, *castitas* in Latin, and *chastity* in English, are equally applicable to a virgin as to a married woman. DIANA is celebrated for her chastity as well as PENELOPE. Some of the Doctors have even limited the application of the term *Chastity* to such as are unmarried. “*Castus et continens*,” says AQUINAS, “*sic differunt, quod castus dicitur ante nuptias, continens verò post eas*.”

BUT,

BUT, says the Abbé de SADE, had LAURA been a virgin, the poet would, in this composition, have given her virgins for her attendants, and not married women. Those who follow her to the Temple of Chastity, says he, are all married women, with the exception of a *Vestal Virgin*. Here our author is guilty of a gross misrepresentation. In the poem of the *Trionfo della Castità*, PETRARCH, so far from citing examples only of married women, as LUCRETIA, PENELOPE, &c. says:

*Io non perria le sacre benedette  
Vergini cb' iui fur chiuder' in rima.—*

“ I could not comprehend in rhyme all the sacred virgins that  
“ were present :” and he enumerates the nine muses,

—CALLIOPE e CLIO con l' altre sette \*;—

together with VIRGINIA and the Vestal TUCCIA. We have here an  
Y-2 example

\* I AM well aware of the doubt that has been entertained by certain grave and learned authors, with regard to the *virginity* of the Muses. As to CALLIOPE, indeed, the matter was past a doubt; for her amour with CEAGRUS, king of Thessaly, was proclaimed by the birth of a son, who made sufficient noise in the world, the famous ORPHEUS; and therefore BUCHANAN has very guardedly expressed himself, with respect to her, in his epigram:

*CALLIOPE longum cælebs cur vixit in ævum?  
Nempe, nihil doti quod numeraret, erat.*

For this lady, though a mother, was certainly *cælebs*, or unmarried; and this is quite sufficient to refute our author's assertion that the poet had here given LAURA an attendance only of married women. As to the other Muses, whatever may have been their failings in private, (and every one of them has suffered from the breath of scandal; see *Menagiana*, t. 2.), their state of *celibacy* is authenticated beyond all question; and I must in conscience believe, that PETRARCH had never heard any of those curious and secret anecdotes of the lives of those ladies, which the penetrating re-  
search

example of unpardonable disingenuity in our Abbé, which, occurring in an instance of easy detection, must justly render suspicious many of those authorities which he pretends to have drawn from private sources, such as the archives of his own and other families, where it is impossible for others to follow him, and investigate the truth of his information.

BEFORE I leave this argument I must observe, that, by adopting our author's own mode of reasoning, the works of the poet will furnish us with similar evidence, directly destructive of his hypothesis. If the state of LAURA, whether married or unmarried, is to be determined from that of her companions or attendants, we find, in many other passages of the poetical works of PETRARCH, that he assigns to her an attendance of virgins. Thus, in the 11th eclogue, in which the companions of LAURA lament her death, under the name of GALATEA, one of them says:

*Addam perpetuos celebret quos mundus honores;  
Virgineos addam cœtus, ritusque verendos.*

And in the 3d eclogue, employing his favourite allusion, the verdant Laurel, under which he always figures his mistress, he says:

*Purpurea in ripa, Laurique virentis ad umbram,  
Virgineam aspicio, cœlo plaudente choream.—*

510, THE last of the arguments advanced by the author of the *Mémoires*, which he gives as in a manner conclusive on this point, is that which is drawn from the dialogue with St AUGUSTINE, and consists in the interpretation given to the contracted

search and deep erudition of modern authors has brought to light, otherwise he would not have made them the attendants of his LAURA, married or unmarried, to the Temple of Chastity.

tracted word *ptbs*, which is found in some manuscripts of the works of PETRARCH. Our author having frankly enough acknowledged that all the preceding arguments amount only to conjectures, (*ce ne sont là après tout, que de très fortes conjectures,*) might certainly have included the last with equal propriety under the same denomination. His interpretation of the word *ptbs* is evidently nothing more than conjecture; to support which we have only his own opinion, and, as he says, that of Messieurs CAPPERONIER, BOUDOT and BEJOT of the King's Library; although, among the *Pièces justificatives*, we find only the certificate of one of those gentlemen (CAPPERONIER) to that effect; and this expressed with such obscurity and confusion of idea, that we cannot tell what are the characters in the two manuscripts he mentions\*. But one thing is plain; before we can admit any conjectural interpretation of this contraction, the Abbé de SADE must prove, that the two manuscripts which bear this contracted word are the oldest of all the manuscripts of the writings of PETRARCH, otherwise his argument concludes nothing; for, if the more ancient manuscripts have the word

at

\* " CERTIFICAT de Monsieur CAPPERONIER, Garde de la Bibliothèque du Roi.

" JE soussigné, Garde de la Bibliothèque du Roi, certifie, que dans le manuscrit du Roi, cotté 6502, contenant un ouvrage de PETRARQUE, intitulé, "*De consiliis curarum propriarum, ad AUGUSTINUM,*" fol. 13. cot. I. on lit, et qu'on doit lire: "*Et corpus illud egregium morbis ac crebris partibus exhaustum multum pristini vigoris amisit;*" lesquels mots se trouvent encore dans le manuscrit cotté 6728. cod. 19. pag. 1. où ils doivent être lus de la même manière. En foi de quoi, j'ai signé le présent certificat, en l'hôtel de la Bibliothèque du Roi, ce 16 Juin 1762.

CAPPERONIER."

It does not appear from this certificate that the two manuscripts mentioned bear any contraction of this word at all; yet these are certainly the manuscripts to which the Abbé de SADE here refers as bearing *ptbs*. When M. CAPPERONIER, therefore, declares, *on lit, et on doit lire*, so and so, the expression is as obscure and inaccurate, as the decision is dogmatical and presumptuous.

at full length, *perturbationibus*, the abbreviation in the later manuscripts must be explained in that manner alone. Now, the Abbé has not taken upon him to maintain, that those two which bear the contraction *ptbs* are the most ancient of all the manuscripts; and his silence on that material point is a presumption of his knowledge to the contrary. But, even were we to allow that those two manuscripts were actually proved to be the most ancient, let us see what could thence be concluded. All the other manuscripts, which we shall suppose were copied from those two most ancient ones, and all the editions of the printed works, have interpreted this contracted word, and written it at full length, not *partubus*, as M. de SADE would have it read, but *perturbationibus*. Now, it will not certainly be denied that the writers of those old manuscripts, and the editors of the oldest printed editions, of the former of whom many were probably cotemporary with the author, and of the latter all lived at no great distance of time from his age, were much better able to read and to interpret the abbreviations of the oldest manuscripts, than those critics who have examined them at the distance of four hundred years from the time when they were written. Even admitting, therefore, that the Abbé had proved his two favourite manuscripts to be the most ancient of all extant, and that the question were simply, What is the most natural interpretation to be given to this contraction? we have, in opposition to his opinion, and that of his friends of the King's Library, the direct authority of all the other manuscripts of the works of PETRARCH, and all the printed editions, that the word ought to be read not *partubus* but *perturbationibus*.

BUT I shall make the author of the *Mémoires* a still more important concession; and I shall even put the case, that instead of two manuscripts, perhaps out of two hundred, which read the word contracted thus *ptbs*, while the rest have it at full length *perturbationibus*, the whole existing manuscripts of the author's works had borne

borne the word thus contracted; and that the question were, Whether it ought to be explained *partubus* or *perturbationibus*? This question, it must be allowed, would be best resolved by considering the sense of the context. Let us now, therefore, see what is thence to be drawn. PETRARCH, in the dialogue in question, feigns a conversation between himself and St AUGUSTINE, in which the latter is endeavouring to convince him of the impropriety of abandoning himself to the influence of a passion, whose imperious power had enslaved to itself, and rendered subordinate, every feeling of his nature. Among other arguments, he urges the folly of setting his whole affection on an object of which death might so soon deprive him. PETRARCH answers, that he hopes he shall never live to see that day; and observes, that, in the course of nature, LAURA being the younger, ought to survive him. To this St AUGUSTINE replies, that such an event is nevertheless the most probable: "*quod corpus illud egregium morbis ac crebris ptbs exhaustum multum pristini vigoris amisit.*" Whether ought we, along with all the editors of the printed works of PETRARCH, to explain this contraction by *perturbationibus*, frequent disquietude or conflicts of mind; or, with the Abbé de SADE, to suppose it to mean *partubus* or child-bearing? The answer immediately made by PETRARCH, must resolve the doubt. "It is probable," says St AUGUSTINE, "that LAURA, though the younger of the two, will soonest be the victim of death; for her constitution, by much sickness, as well as <sup>mental inquietude,</sup> <sup>child-bearing,</sup> has lost a great deal of its former vigour."—"I too," says PETRARCH, "have had my share of mental inquietude, and that more severe than hers, and I am considerably more advanced in life: *Ego quoque et curis gravior, et atate provectior factus sum.*" If we admit the common reading, the reply is rational and consonant: "I have suffered from the same cause, and more than she has." If we adopt the interpretation:

pretation of the Abbé de SADE, the reply is equally absurd and impertinent to the observation that precedes it.

WITH regard to the critical decision pronounced by the Abbé on the meaning of the word *creber*, viz. that it imports a repetition of *acts*, and that it cannot with propriety be applied to *passions*; it had certainly been proper that he had supported this judgment either by some authorities of professed grammarians, or examples from classical writers. He has done neither; and that for the best of reasons: he had none to produce. *Creber*, as we find from the best authorities, is used precisely in the sense of *frequens* or *assiduus*; and is therefore, with perfect propriety, applied as well to passions as to acts. He observes, that had *perturbationibus* been the proper reading, the author would have coupled it with *multis*, and not with *crebris*; a remark betraying ignorance of grammatical precision: for who is there that needs to be told that *multus*, applying to number, can with no propriety be employed to denote frequency of repetition?

BUT the author of the *Mémoires* asks, What passions we can suppose to have exhausted the constitution of the most *prudent* and *modest* of women, who led a life so *simple* and so *uniform*? To this I answer by another question: How can we, ignorant as we are of the private and domestic history of this lady, pretend to say what causes she might, or might not have had, of anguish and disquiet? How many women of prudence and of modesty are, from unavoidable circumstances of situation, the victims of mental inquietude; and experience, even in a life of the utmost privacy and retirement, the keenest anguish, from the turbulent passions, the malice or the caprice, of those with whom they are connected.

I HAVE now, as I trust, impartially canvassed the whole of those arguments drawn by the author of the *Mémoires* from the  
works



works of PETRARCH himself, or what may be termed the intrinsic evidence in support of the material part of his hypothesis, namely, that LAURA was a married woman; nor do I think I presume too much when I say, that I have shewn their absolute insufficiency to prove that proposition. The question might therefore be safely left here; in the confidence, that an hypothesis which is newly brought forward\* in opposition to long established belief, and the concurring assent, for ages, of all who have been conversant with the matter of inquiry, and which is shewn to rest on no basis, either of historical evidence or of sound reasoning, does not require the confutation of opposite proof to lay it in the dust: But, as I have already had occasion incidentally to produce some of those testimonies from the works of the poet himself, which tend most positively to disprove this hypothesis, and an abundance of matter of similar import must offer itself to all who have examined those works with any attention, I am prompted to draw yet a little more from this stock of the internal evidence, before I take my leave of the subject.

imò. PETRARCH has composed 318 sonnets, 49 *canzoni* or songs, and 6 *trionfi*; a large volume of poetry, entirely on the subject of his passion for LAURA; not to mention a variety of passages in his prose works, where that favourite topic is occasionally treated, and even discussed at very great length. In the

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whole

\* THIS hypothesis may certainly be termed a new one; since, although TASSONI, and some of the commentators on the Sonnets of PETRARCH have, in their observations on certain passages which the Abbé de SADE produces as proofs of his theory, remarked, that a suspicion might thence arise that LAURA was a married woman, none of them have ventured to affirm (as our author) for certain, that she was so. On the contrary, VELUTELLO's conclusion, after confessing the very imperfect information which could be collected relative to the family, state and condition of LAURA, is: "*Per cosa certa habbiamo da tenere ch' ella non fosse mai maritata.*"—"We must hold it for a point absolutely certain, that she was never married."

whole of those works, there is not to be found a *single passage*, which intimates that LAURA was a married woman. Is it to be conceived that the poet, who has exhausted language itself in saying every thing possible of his mistress; who mentions not only her looks, her dress, her gestures, her conversations; but her companions, her favourite walks and her domestic occupations, would have omitted such capital facts, as her being married, and the mother of many children; married too, as the author of the *Mémoires* asserts, to a man who was jealous of her, and who used her with harshness and unkindness on PETRARCH'S account?

2dò. WOULD this harsh and jealous husband have permitted this avowed admirer of his wife, this importunate gallant, who followed her as her shadow wherever she went, and attended her in town and in the country, to see her daily, and converse with her alone, to write to her, to make assignations with her, and to send her presents as tokens of his attachment? Yet, that PETRARCH enjoyed all these liberties, is evident from numberless passages of his works\*. That the poet and his mistress were wont even to walk together in the public gardens, is evident from the incident alluded to in the 208th sonnet, *Due rose fresche*: A friend, who met them together in a garden, taking them both by the hand, presented each with a rose, declaring, at the same time, that the sun never shone on a truer pair of lovers. And that their passion was the common discourse

\* SON. 41. *Perchè io t' habbia.* Son. 59. *Quando giunse.* Canz. 14. *Perchè quel.* Canz. 15. *Volgendo gli occhi.* Son. 49. *Se voi poteste.* Canz. 12. *Perchè al viso.* Canz. 4. *Nel dolce tempo.* Son. 19. *Mille fate.* Canz. 8. *Si è debile.* *Trionf. di Morte*, cap. 2. &c. &c. In the 42d sonnet, (*Se col cieco desir*), PETRARCH complains that LAURA had failed to keep an appointment that she had made with him, and in which, he had flattered himself, he was to be indulged in freely declaring his passion.

discourse of the public, appears from many passages, where the poet dwells on that circumstance as a matter of regret :

—Sì comè al popol tutto  
Favola fù gran tempo, onde sovente  
Di me medesimo meco mi vergogno.— Son. I.

3<sup>ti</sup>ò. PETRARCH, in the 200<sup>th</sup> sonnet, (*Real natura*)\*, records the following remarkable anecdote. At a brilliant assembly and festival, given on occasion of the arrival of a foreign prince at Avignon, LAURA was present, along with the most distinguished ladies of the place. This prince, whom the poet celebrates as a most amiable and accomplished character, curious to see a lady of whom the compositions of PETRARCH had given him so high an idea, eagerly sought her out amidst the crowd, and soon discovered her by her superior beauty and the gracefulness of her demeanour. Approaching her with an air of gentleness united with dignity, and making a sign to the ladies who surrounded her to stand a little apart, he took her by

Z. 2. the:

\* *Real natura, angelico intelletto,  
Ghiar' alma, pronta vista, occhio cerviero,  
Provvidenza veloce, alto pensiero,  
E veramente degno di quel petto ;  
Sendo di donne un bel numero eletto  
Per adornar il dì festo ed altero,  
Subito scorse il buon giudicio intero  
Fra tanti, e sì bei volti il più perfetto ;  
L' altre maggior di tempo, o di fortuna  
Trarsi in disparte comandò con mano,  
E caramente accolse a sè quell' una :  
Gli occhi, e la fronte con sembiante umano.  
Bacciolle sì, che rallegrò ciascuna :  
Me empì d' invidia l' atto dolce e strano.*

the hand, and (after the fashion of his country) saluted her, by kissing her forehead and her eyes: a mark of regard, says the poet, which was approved of by all the ladies who were present, but which he himself beheld with envy. The commentators are not agreed as to the prince of whom this anecdote is recorded. M. de la BASTIE is of opinion that it was ROBERT, King of Naples, who is known to have distinguished PETRARCH by many marks of friendship and beneficence, and whom the poet has, in various parts of his writings, celebrated with the highest eulogy: and this is likewise the opinion of BEMBO, DANIEL, and others. The Abbé de SADE, on the other hand, has adduced some strong arguments to shew, that the prince here alluded to was CHARLES of Luxembourg, son of JOHN, king of Bohemia. The dispute, as to the persons, is of no consequence; the anecdote must be admitted as true, and it has ever been regarded as highly honourable both for the poet and his mistress. In that light we are assured it was considered by the ladies who were present; and, as it is no part of the female character, to view with complacency an unmerited preference shewn to a rival in beauty or accomplishments, we must hold this as an unequivocal proof, that they considered this flattering mark of distinction as deservedly bestowed, and, of course, that they regarded the attachment of PETRARCH and of LAURA as an honourable and virtuous flame. Now, let it be supposed, with the Abbé de SADE, that this lady, thus highly distinguished as the object of the poet's passion, had been the wife of a man of rank and character, the mother of a family, is it possible to believe that this foreign prince, who is described as a paragon in every courtly accomplishment, should have thus openly braved every law of decency and of propriety, and, in a full assembly (met to do him honour) have insulted, not only the husband of this lady, but every woman of honour or of virtuous character

rafter who was present? Is it to be conceived, that the husband of this lady, strongly impressed with the feelings of jealousy on the score of this ardent attachment, as this author himself represents him to have been, and who, in all probability must himself have witnessed the incident here recorded, should have silently and tamely submitted to this gross affront? Is it possible to figure, that the whole assembly should have crowned with their approbation this glaring indignity and violation of decorum?

4<sup>th</sup>. WOULD this jealous husband have not only patiently witnessed the mutual expressions of this ardent passion for the space of twenty-one years, that his wife was alive; but have complaisantly permitted her gallant, or a friend under his character, to embalm the memory of his mistress by a rapturous love-elegy, to be inclosed in her coffin; the last insult which the honour of a husband could sustain? Yet this, we must believe, if we adopt the hypothesis of the Abbé de SADE: For, if the story of the leaden casket has any truth in it at all, (and its supposed truth is the main prop of that hypothesis), this elegy or sonnet must have been written, either by PETRARCH himself, or by a friend assuming his character.

5<sup>th</sup>. AN amour of this kind, with a married woman, the mother of a numerous family, under whatever colours this reverend author, in the laxity of the morals of his country may choose to palliate and disguise it; was in itself an offence both against religion and morality, and must have been viewed by the poet himself in a criminal light. But the general morals of PETRARCH were exemplary, his virtue was even of a rigid cast; and, if at any time he was overpowered by the weakness of humanity, his mind, naturally of an ingenuous frame, suffered the keenest contrition, and prompted to an ample atonement, by a sincere avowal of his fault. In this light, however, he never considers

considers his passion for LAURA. On the contrary, it appears to have been his glory and pride, and to have exalted him equally in his own esteem and in that of others.

*Anima—*

*Dà lei ti vien l'amoroso pensiero;  
Che mentre 'l segui al sommo ben t' invia,  
Poco prezando quel ch' ogni huom desia:  
Da lei vien l'animoſa leggiadria,  
Ch' al ciel ti ſcorge per deſtro ſentero;  
Si ch' i vo già de la ſperanza altero.* Son. 12.

Hence has my ſoul her nobleſt aims deriv'd,  
From that pure flame; hence rais'd her thoughts on high,  
Indignant, ſpurning what the vulgar train  
Of earth-born ſpirits prize. O beſt of guides,  
That chear'ſt with hopes, that proudly liſt the ſoul,  
And point the path to heaven! —————

“ In amore meo,” ſays PETRARCH, in his dialogue with St AUGUSTINE, “ nil turpe, nil obſcœnum, nil denique præter magnitudinem culpabilis.” *Dial. de contemptu mundi.* “ Illa juvenilem animum ab omni turpetudine revocavit, uncoque retraxit, atque alta compulſit ſpectare.” *Ibid.* “ Amore acerrimo, ſed unico et honeſto, in adoleſcentia laboravi, et diutius laborâſſem, niſi jam tepelcentem ignem mors acerba, ſed utilis, extinxiffet.” *Epift. ad poſt.*

To any perſon who is acquainted with the writings of PETRARCH, and eſpecially with thoſe which were compoſed after the death of LAURA, it muſt appear the moſt bigotted perverſion of ideas, to maintain that they are conſiſtent with the notion of his cheriſhing a paſſion for a married woman. I ſhall here tranſlate a few paſſages from thoſe latter poems.

IN

IN the 289th sonnet, or the 61st of the second part, written after the death of LAURA, we find these strong expressions :

*S' honesto amor, può meritar mercede \**, &c.

“ If honourable and virtuous love e’er merited a reward, and  
 “ if compassion can aught avail, I shall obtain the recompence  
 “ of a constancy of affection, which, towards that dear object,  
 “ and in the eyes of the world, was as pure as the light of  
 “ heaven. Formerly she mistrusted that affection, and was un-  
 “ certain of the end and object of my passion. Now she sees  
 “ my heart and inmost soul; and thence I trust that in heaven  
 “ she now compassionates my sufferings: for oft I behold her  
 “ in my dreams, regarding me with looks full of tenderness and  
 “ pity; and I fondly hope, that when I too shall have laid aside  
 “ this garb of mortality, she will welcome me to those blest a-  
 “ bodes, where all true followers of CHRIST, and friends to virtue,  
 “ shall dwell for ever in happiness.” Can any person who reads  
 this remarkable passage, believe that the love of PETRARCH was  
 a criminal and adulterous affection? If he still hesitates on that  
 point,

\* *S' honesto amor può meritar mercede :*  
*E se pietà anchor può quant' ella suole,*  
*Mercede havrò ; che più chiara che 'l sole,*  
*A madonna ed al mondo è la mia fede.*  
*Già di me paventosa, hor sa, nol crede,*  
*Che quello stesso, ch' or per me si vuole,*  
*Sempre si volse ; e s' ella udia parole,*  
*O vedea 'l volto, hor l' animo, e 'l cor vede.*  
*Ond' i' spero che 'n fin al ciel si doglia*  
*Di miei tanti sospiri ; e così mostra*  
*Tornando a me sì piena di pietate :*  
*E spero ch' al por giù di questa spoglia*  
*Venga per me con quella gente nostra*  
*Vera amica di CRISTO, e d' honestate.*

point, let him read the following striking apostrophe in sonnet 302.:

*Donna, che lieta \*, &c.*

“ O Lady, that now standest in the presence of God ; as, sure  
 “ thy spotless life has well deserved that place ! O matchless pa-  
 “ ragon of all that is excellent in woman ! Now, with the eyes  
 “ of Him who sees all things, thou beholdest my faithful love,  
 “ my pure and virtuous affection ; and thou seest, that towards  
 “ thee my heart felt the same emotions while thou wert on  
 “ earth, as now in heaven. O, that, in reward of all my length-  
 “ ened sufferings, I soon may join thee there !”

IN the end of sonnet 303., immediately following, he thus expresses himself :

*Sol un conforto †, &c.*

“ My only comfort is, that she who sees my thoughts, may obtain for me that mercy that I may soon be with her.”

So,

\* Donna, che lieta col principio nostro

Ti stai come tua vita alma richiede,

Affisa in alta e gloriosa sede.—

O delle donne altero e raro mostro,

Hor nel volto di lui che tutto vede,

Vedi 'l mio amore, e quella pura fede.—

E senti, che ver te il mio core in terra

Tal fu, qual hora in cielo.—

Dunque per ammendar la lunga guerra,

Per cui dal mondo a te sola mi volsi,

Prega, ch' io venga tosto a star con voi.

† Sol 'un conforto a le mie pene aspetto

Cb' ella che vede tutti i miei pensieri

M' impetre gratia ch' i' possa esser seco.



So, likewise, in the next sonnet, 304. \*.

“ O happy day, when, issuing from this earthly prison, and  
 “ throwing off the spoils of mortality, bursting from this cloud  
 “ of darkness into the splendour of eternal day, I see at once  
 “ my GOD, and the dear object of my love !”

IN his hymn to the Virgin, with which he concludes his sonnets, and which is, perhaps, the most perfect of his compositions, where he confesses all the errors and weaknesses of his life ; and when, from the nature of the subject, he must have deemed it nothing less than impiety to have uttered a falsehood, or even to have palliated or extenuated a crime, he takes merit to himself, in the sight of heaven, for his passion for LAURA ; and thus reasons with the Blessed Virgin, appealing to that clemency which he supposes her peculiar characteristic † :

“ O Blessed Virgin, paragon of clemency and humanity,  
 “ let the example of the Almighty Being incite thee to shew  
 “ mercy to an humble contrite heart ; and if with such strength  
 “ and ardour of affection I have been capable of loving a frail  
 “ mortal, what mayst thou not conclude must be my devotion  
 “ towards

\* *O felice quel dì, che del terreno  
 Carcere uscendo, lasci rotta e sparta,  
 Questa mia grave, e frale, e mortal gonna.—  
 E da sì folte tenebre mi parta,  
 Voltando tanto su nel bel sereno,  
 Ch' io veggia il mio SIGNORE, e la mia Donna.*

† *Vergine umana, e nemica d' orgoglio,  
 Del commune principio amor t' induca ;  
 Miserere d' un cor contrito umile ;  
 Che se poca mortal terra cadauca  
 Amar con sì mirabil fede soglio,  
 Chè dourò far di te cosa gentile !—* Canz. 8. Part. 2.

“ towards Thee, the bright example of all excellence !” In perfect union with these sentiments of the poet himself, is that beautiful sonnet, which BOCCACCIO, recently after the death of PETRARCH, composed in memory of his departed friend. It is formed on that favourite thought, which is most natural to the soul on the near prospect of death :

*Or se' salito, caro Signor mio,  
Nel regno al qual salir ancora aspetta  
Ogni anima da Dio a quello eletta,  
Nel suo partir di questo mondo rio.  
Or se' cola dove spesso il desio  
Ti tirò già per vedere LAURETTA ;  
Or se' dove la mia bella FIAMMETTA  
Siede con lei nel cospetto di Dio.  
Or con SENUCCIO, e con CINO, e con DANTE,  
Vivi sicuro d' eterno riposo,  
Mirando cose da noi non intese.  
Deh ! s' aggrado ti fui nel mondo errante,  
Tirami dietro a te, dove gioioso  
Vegga colei che pria d' amor m' accese.*

“ Now art thou gone, my dear master, to those blissful seats,  
“ to which all the spirits of the just, the elect of God aspire,  
“ when freed from this abode of guilt. Now hast thou attained  
“ that heavenly region, which thy longing desire to see thy  
“ departed LAURA had made the object of thy most earnest  
“ wishes ; where my lovely FIAMMETTA sits, along with her, in  
“ the presence of GOD. Now, with SENUCCIO, with CINO, and  
“ with DANTE, thou livest in the blissful assurance of eternal  
“ rest ; and delightest in the contemplation of those things  
“ which surpass our weak understanding. Oh ! if I was ever  
“ dear to thee while in this fleeting scene of existence, draw me  
“ now to thyself, where once more I may be happy in the sight  
“ of her who was my first, my only love !”

6<sup>to</sup>. IN the dialogue above quoted, (*De contemptu mundi*, Dial. 3.), where St AUGUSTINE is introduced reasoning with the poet, and endeavouring to convince him of the errors of his past life, and particularly to dissuade him from the indulgence of his passion for LAURA, to which he was as much a slave after her death as he had been during her life, the holy father makes use of every argument that can be drawn both from religion and morality: Would he have omitted the strongest of all arguments? Would he have forgotten to urge that LAURA was the wife of another; and, consequently, that his passion was a crime both in the sight of God and man? Yet, to this purpose, there is not a single word in the whole of that various and labour-ed argument.

7<sup>mo</sup>. IT will be in vain for a disciple of this new hypothesis to attempt its justification, upon the principle that the love of PETRARCH, being entirely of a refined and Platonic nature, might innocently have for its object a married woman, and the mother of a family. The author of the *Mémoires* himself abandons that ground of argument\*, which, indeed, cannot be maintained in any consistency with those sentiments which the poet himself has avowed in many parts of his works. The love of PETRARCH was no otherwise distinguished from an ordinary passion than by its fervency and duration. He felt for LAURA the same emotions, which an ardent but honourable lover feels for a most beautiful, amiable and accomplished mistress. He admired the graces of her mind, he revered her virtues, and he was enamoured of the beauties of her person. He owns, that he passionately desired the reward of his love in the possession of this treasure. The poet, who expresses himself thus rapturously in the language of ordinary human love, must abandon all pretensions to a Platonic affection:

A a 2

Con

\* *Mémoires pour la Vie de PETRARQUE*, vol. i. note 21.

*Con lei foss' io \**, &c.

" Would that I were with her but a single night, alone, in darkness, where only the stars should behold us ; and might that night last for ever ! Might she never assume a borrowed shape, (like DAPHNE), to escape from my arms !"

*Deb bor foss' io con vago de la LUNA†*, &c.

" Ah that I were along with ENDYMION, the beloved of LUNA, laid to rest in some green wood ; and that she who consumes my fleeting days were there alone with me, or attended only by love, for one single night ! But might the fun remain for ever beneath the waves !"

So in the 58th sonnet,

PIGMALION, *quanto lodar ti dei* ‡, &c.

" How much, O PIGMALION, hadst thou reason to be content  
" with

\* *Con lei foss' io da che si parte il sole !  
E non ci vedess' altri, che le stelle,  
Sol una notte ; e mai non fosse l' alba,  
E non si trasformasse in verde selva  
Per uscirmi di braccio. — Canz. 3. A qualunque, &c.*

† *Deb bor foss' io con vago de la LUNA  
Addormentato in qualche verdi boschi  
E questa ch' anzi vespro a me fa sera  
Con essa, e con amor in quella spiaggia  
Sola venisse a stars' ivi una notte !  
E' l di sì stesse, e' l sol sempre ne l' onde !*  
Sest. 7. Part. 1. *Non ha, &c.*

‡ PIGMALION, *quanto lodar ti dei  
Dell' imagine tua, se mille volte  
N' avessi quel ch'io sol una vorrei.*

“ with thy image, who enjoyedst a thousand times those de-  
lights, which O that I but could once taste\*.”

THESE passages leave no room for the supposition of that re-  
fined and Platonic affection, which it is pretended a virtuous  
young man may, without blame, indulge for the wife of ano-  
ther. I enter not into the question, how far even a theory of this  
kind is reconcileable to strict morality, or whether that species  
of continued attention, that marked esteem and preference,  
which at least must be rewarded by a corresponding sympathy  
and regard for the person who expresses them, is materially less  
injurious to the sacred bond of conjugal affection, than a plan  
of seduction pursued from its ordinary motives. A moralist  
might perhaps decide, that where the effect of both is the same,  
the alienation of the affections of a wife; the garb of virtue and  
of decency, assumed by the former, is only a higher aggravation  
of its criminality. But the discussion of this question is super-  
fluous, where the supposition of a Platonic love cannot, as we  
have seen, be admitted.

8<sup>vo</sup>. Lastly, As the love of PETRARCH for LAURA was an  
honourable and virtuous passion, so the works of the poet afford  
sufficient evidence, that he ardently desired to be united to  
LAURA in marriage, and was even in the near prospect of that  
happiness :

*Amor con quanto sforzo oggi mi vinci!*

*E se non ch' al desio cresce la speme;*

*I cadrei morto, ove più viver bramo.* Son. 64.

Già

\* It is amusing to observe, how even this passage has been strained to admit of  
an interpretation suited to that Platonic affection which some of his commentators  
have wished to ascribe to the poet. The pleasures, say they, which PETRARCH  
here expressed his desire of enjoying, were those which would arise on finding the  
picture of LAURA endowed, like PIGMALION's ivory image, with speech and under-  
standing. But they own, at the same time, that, as PIGMALION's enjoyments are  
generally believed to have been less refined, the poet has chosen an unlucky allusion ;  
and that the obvious sense of the passage is *rien moins que Platonique*.

Già incominciava a prender securtade  
 La mia cara nemica a poco a poco  
 De' suoi sospetti ; e rivolgeva in gioco  
 Mie pene acerbe, sua dolce honestade :  
 Presso era 'l tempo dov' amor si scontra  
 Con castitate ; e a gli amanti è dato  
 Sederfi insieme, e dir che lor' incontra. Son. 47. Part. 2.

Tempo era homai da trovar pace o tregua  
 Di tanta guerra ; ed erane in via forse. Son. 48. Part. 2.

Tranquillo porto avea mostrato amore  
 A la mia lunga e torbida tempesta.—  
 Già traluceva a' begli occhi 'l mio core,  
 E l' alta fede non più lor molesta.  
 Abi morte ria, come a scbiantar se' presta  
 Il frutto di molt' anni in sì poche bore ! Son. 49. Part. 2.

IN supplement of these authorities from the sonnets of PETRARCH, may be added that report which was current at the time, or at least among the earliest writers who have given any account of the poet's life, namely, that the Pope, who held PETRARCH in the highest estimation, and to whom he was indebted for several valuable ecclesiastical preferments, was extremely folicitous that he should be united in marriage to LAURA, and offered to give him, in that event, a dispensation for retaining his church-benefices. If the story is true, the Pope of whom it is recorded must have been CLEMENT VI., as he is the only one of the Pontiffs, who were PETRARCH's coterporaries, to whom these characteristics could apply. M. FLEURY, in his *Ecclesiastical History*, is certainly mistaken when he attributes the proposition here mentioned to BENEDICT XII. the predecessor of CLEMENT ; for PETRARCH owed no favour to that Pontiff, whom he has satirized in many parts of his writings, as a  
 man

man of barbarous dispositions, mean and sordid propensities, and whose gross ignorance disgraced the high station which he occupied. The anecdote is mentioned by SQUARZAFICHI; as it is likewise by one of the oldest editors of the poems of PETRARCH, in the preface to the Venice edition 1473: though the latter author erroneously names the Pope URBAN V. who did not ascend the papal chair till some years after the death of LAURA. This uncertainty with regard to the person is not, however, sufficient to discredit the fact itself, that the Pope, whoever he was, might, from favour to PETRARCH, have earnestly desired to see him united to the object of his passion.

THE clerical character of PETRARCH ought not to be considered as affording any objection to the supposition, that he ardently wished to be united to LAURA in marriage. Though enjoying ecclesiastical preferments, he had never accepted of any charge which conferred a care of souls. He had frequently been solicited with earnestness to accept of a bishopric; but constantly refused it, either from a sense of his own demerits, when weighed against the qualities he thought requisite for that sacred character, or, more probably, (as he himself indeed hints), from a desire to preserve his liberty, and follow, without restraint, that course of life which he found most congenial to his taste. It is probable, therefore, that his views with respect to LAURA had their influence on this determination; since he was thus at liberty, merely by the sacrifice of some slender pecuniary emoluments, to change his condition at any time he might think proper. Examples of this kind were at that time extremely common; and the story above related, if true, is a proof that the Sovereign Pontiffs were even in use to dispense with the resignation of benefices to their particular favourites in those circumstances.

BUT, whatever weight we may be inclined to give to this anecdote, it is, on the whole, sufficient to our purpose, if while, on the one hand, we have shewn that there is not the smallest  
solidity

solidity in all that elaborate fabric of argument, which has been brought to prove that LAURA was a married woman, we have proved, on the other, from the whole tenor of the writings of PETRARCH, the only evidence that applies to the matter, that his affection for LAURA was *an honourable and virtuous flame*. On this subject the reflection of M. de la BASTIE is equally beautiful, as a sentiment of morality, and just, as an observation on human nature: “ *Il n’y a que la vertu seule qui soit capable de faire des impressions que la mort n’efface pas.*”



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VII. DESCRIPTION of an EXTRA-UTERINE FŒTUS. By Mr  
THOMAS BLIZZARD, F. R. S. EDIN. *Lecturer on Anatomy  
and Surgery, and Surgeon to the London Hospital.*

[Read Feb. 3. 1800.]

MUCH light has been thrown upon many of the most important and intricate functions of the animal machine, by accurate inquiries into the processes of nature under extraordinary circumstances. The doctrine of conception, lately advanced, owes its support, in no small degree, to observations made on cases, in which, from adventitious causes, Nature has been led out of her ordinary tract. The following case appears to be of such a description; and, as tending further to elucidate the subject, may probably be not unacceptable to the learned members of this Society.

THE woman, who was the subject of the following investigation, was aged twenty-eight, of a robust form; had been six times pregnant; first at the age of nineteen, when she was delivered of a living child; she then had two abortions; of the fourth pregnancy she was delivered of a living child; and of the fifth she miscarried, about five weeks before her death. On the morning of the day she died, at about half past eleven, after having gone through considerable fatigue in cleaning her house, she was seized with a violent pain in the lower part of the abdomen, which continued till her death; she gradually became weaker; her abdomen became more and more swollen, and about nine in the evening she died. Having felt extraordinary

nary sensations for a short time before this attack, she expressed to her friends a great desire, that, should she die, her body might be examined.

THE abdomen being opened, there appeared, proceeding from the pelvis, a considerable quantity of blood, both in a fluid, and in a coagulated, state: two measured quarts were taken from the cavity.

THE source of the hæmorrhage was an interesting point of inquiry. This soon appeared. In the middle of the Fallopian tube of the left side, nearly at an equal distance from its fimbriated extremity and its termination in the uterus, a pouch was formed, about the size of a pigeon's egg, or a little larger, of an oval form, in the middle of which was a rupture, of the bigness of a small quill, through which it was plain the blood had issued. The parts were removed, for more accurate examination. Having divided the vagina, I was struck with the appearance of a quantity of jelly, extending from the os uteri, and also with the increased size of the uterus, which led to a suspicion that there had been a recent conception. The nature of the case now began to unfold itself. It appeared, that an extra-uterine gestation had taken place; that the process was going on in the Fallopian tube, the embryo having rested there instead of passing to the cavity of the uterus; that the tube had enlarged to the capable extent, and then had burst. The fimbriated extremity of this tube was open as usual, and its cavity nearly of the natural size, perhaps a little enlarged. It possessed also the tortuous form, as is common, till it began to expand to form the pouch. The tube was of its natural size also, in that half towards the uterus, and was also pervious; for, quicksilver was introduced by the tube near the uterus, towards the pouch, which passed readily, and insinuated itself between the pouch and its contents. The quicksilver passed readily also into the  
cavity

cavity of the uterus; so that there was no permanent obstruction to the passage of the embryo into the uterus.

HAVING carefully laid open the pouch, it appeared full of a firm coagulum. Upon breaking through this, at the depth of about a quarter of an inch, there appeared a membranous circle, of a lightish colour, about the size of a scarlet bean, and within this another similar membrane presented itself, perfectly distinct from the former; probably these membranes represented the chorion and amnion. Within the latter membrane, nothing could be discovered but coagulum. There was no appearance of foetus; but it must be remembered, that only five weeks had elapsed since the last abortion.

THE uterus was much thickened, being nearly twice its bulk in an unimpregnated state; and, notwithstanding the hæmorrhage which had occurred, its vessels appeared considerably enlarged. The cervix uteri was entirely filled with a jelly, and the whole surface of the cavity of the uterus was lined with a softish glutinous substance, which seemed organized, and was much redder than the rest of the uterus. Changes had been produced in the uterus similar to what are seen in ordinary gestation; its parietes were thickened; its cavity enlarged; its cervix shut up with a jelly; and the glutinous effusion just described might not perhaps improperly be considered as a *membrana decidua* in its early state of formation.

THE ovaria were much vesiculated, particularly the left; in which there was a corpus luteum of considerable size: in the right ovary there was no corpus luteum. The vesicles in these ovaria were large and much distended. Their appearance was exactly similar to that of the ovaria of a young woman I some time ago examined, who had died from an accident, after having been married but a few days, in whose uterus there were evident marks of impregnation, and in both ovaria, corpora lutea, two or three, I am not certain which, in one ovary,

and one in the other. The corpora lutea, in both cases, appeared to be formed by an effusion of blood, afterwards coagulated, into a burst vesicle. The section which shewed the corpus luteum, discovered its internal substance to be dark, with a border of a yellowish colour, as if formed of coagulating lymph, separated from crassamentum.

THE very early impregnation, after the abortion, in this case, is a circumstance that seems entitled to remark. Only five weeks intervened between her last miscarriage and her death; and it must be supposed that impregnation happened a considerable time before her death, from the changes which had taken place. If I might be allowed to venture a conjecture of the cause of these phenomena, Does it not appear that there might have been some irregular contraction of the Fallopian tube, which is probably muscular, that caused the embryo to rest where it did? It was proved there was no permanent cause of obstruction in the tube.

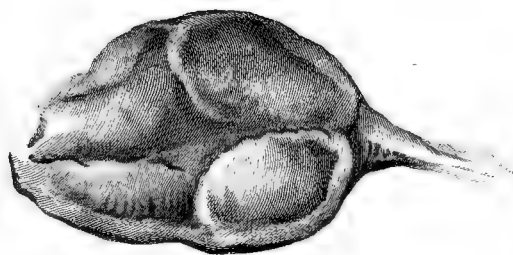
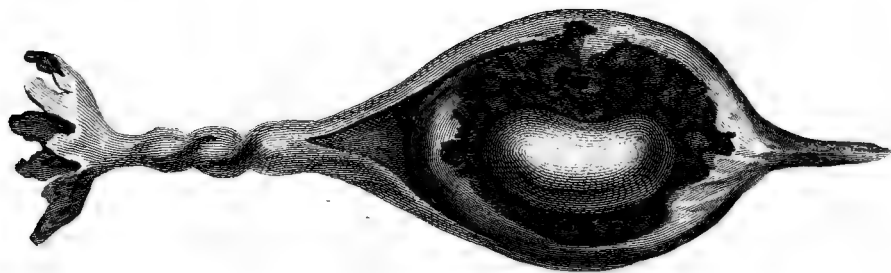
#### EXPLANATION OF THE PLATES.

PLATE II. shews the uterus and its appendages, with the enlargement in the Fallopian tube, having a burst opening in its middle. The whole is drawn, as nearly as possible, of the dimensions of the parts. By the division of the uterus may be seen its increased thickness and size, and an attempt to shew the jelly occupying the cervix uteri.

PLATE III. exhibits two figures: the one shews the enlargement of the Fallopian tube, and the appearance of its contents as described; the other expresses the ovarium of the same side, with a large corpus luteum.











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VIII. METEOROLOGICAL ABSTRACT *for the YEARS 1797, 1798,  
and 1799. Communicated by JOHN PLAYFAIR, F. R. S.  
EDIN. and Professor of Mathematics in the University of  
Edinburgh.*

[*Read at the Meetings in Jan. 1798, 1799, & 1800.*]

**I**N drawing up this abstract, I have somewhat enlarged the plan followed in those formerly communicated to the Society. In order to represent more accurately the progress of the seasons, every month is here divided into three parts, and the state of the barometer and thermometer is given for each of these divisions.

IN the tables, therefore, that follow, the first column contains the greatest height of the barometer for each of the above divisions; the second the least; the third the mean; and the fourth the temperature of the air in the room where the barometer is kept; the fifth and sixth columns shew the greatest height of the thermometer in the air that was observed during the ten days to which the numbers refer; the next three give the mean heights as observed at three different times every day, viz. at 8 in the morning, 10 in the evening, and, as nearly as can be judged, when the day is warmest, that is, some time between mid-day and 3 in the afternoon. The mean of all these three is taken for the mean temperature of the day, which being computed for each day, the mean of all these mean temperatures is set down as the medium temperature of the air for every one of the thirty-six

thirty-six divisions of the year. The mean of the three divisions of every month is given in the next column, under the title of the mean temperature of the month.

THIS arrangement of the meteorological abstract has been adopted, as exhibiting the variations both of the barometer and thermometer, and also their mean state, without either entering into particulars too minutely, or abstracting from them too much.

It is presumed that the mean temperatures, which are the points most difficult to be ascertained, are given with tolerable exactness, as they are deduced from three observations made every day, of which the first, that at 8 A. M. is itself not far from the medium temperature of the whole day, and the other two are as near as circumstances will allow, to the two extremes of greatest heat and greatest cold.

THE barometer of which the heights are here given, is the same that was used formerly, (See *Transactions of the Royal Society of Edinburgh*, Vol. IV. p. 213.) ; but in May 1798 its place was changed from Windmill Street to Buccleugh Place, about 130 yards farther to the south; 3 feet lower than before, or about 262 above the level of the sea. The thermometer is shaded from the sun, exposed to the north, and about 30 feet from the ground: At this height it often stands at 33 or even 34 degrees, when a slight frost is felt on the surface.

METEOROLOGICAL TABLE FOR 1797.

		Greatest Height of Barometer.	Least Height of Bar.	Mean Height of Bar.	Mean Temperature of Merc. in Barometer.	Greatest Height of the Thermometer.	Least height of Ditto.	Mean Temp. of the Air 8 A. M.	Mean Temp. of Air when warmest.	Mean Temp. of the Air 10 P. M.	Mean Temp. of every ten days.	Mean Temp. of the Month.	Depth of Rain.	Days of West wind.	Days of East wind.
		Inches.	Inches.	Inches.	Inches.	°	°	°	°	°	°	°	Inches.		
Jan.	Div. 1.	30.324	29.610	30.0776	48.1	44.5	34.5	39.35	41.10	38.95	39.0				
	— 2.	30.296	29.096	29.7866	44.8	51.5	29.25	37.97	40.12	38.75	38.94	40.38	1.320	5	5
	— 3.	29.670	28.918	29.4400	48.4	52.0	35.0	42.66	44.50	42.50	43.22				
Feb.	Div. 1.	30.330	29.294	29.9302	41.3	52.0	33.0	47.40	49.65	40.30	45.78				
	— 2.	30.126	28.820	29.7742	48.2	52.0	32.0	38.15	42.65	39.85	40.22	43.19	.775	8	2
	— 3.	30.298	30.022	30.1385	51.9	52.0	34.0	40.94	47.93	41.87	43.58				
March,	Div. 1.	30.106	29.245	29.7442	48.1	53.0	31.0	37.0	42.33	39.0	39.44				
	— 2.	30.124	29.238	29.6432	48.5	50.25	34.0	39.3	45.75	41.0	42.08	41.85	1.030	5	5
	— 3.	29.922	29.038	29.5281	49.0	54.5	37.75	42.8	47.0	42.5	44.05			7	4
April,	Div. 1.	30.012	29.315	29.6154	49.75	53.0	39.5	43.12	50.0	42.6	45.24				
	— 2.	29.942	29.276	29.5272	50.3	55.50	38.25	45.52	52.50	46.0	48.01	46.51	1.460	5	5
	— 3.	30.104	29.428	29.6442	51.4	60.0	40.50	44.35	50.02	44.5	46.29			5	5
May,	Div. 1.	29.974	28.970	29.4664	52.3	58.5	37.5	46.1	50.12	42.37	46.19				
	— 2.	29.919	29.220	29.5900	54.9	64.0	39.5	52.0	55.70	51.05	52.92	51.69	2.630	8	2
	— 3.	30.116	29.310	29.6654	59.7	72.5	45.75	55.68	60.38	51.84	55.97			10	1
June,	Div. 1.	29.984	29.112	29.6400	57.4	63.0	43.5	52.67	58.05	47.70	52.81				
	— 2.	29.932	29.432	29.7214	58.9	62.5	44.25	53.10	56.75	50.17	53.34	53.65	1.533	7	3
	— 3.	29.886	29.484	29.7300	59.0	63.0	49.5	54.05	58.72	51.67	54.81			1	9
July,	Div. 1.	30.026	29.186	29.5948	62.4	70.5	53.0	57.92	63.10	57.30	59.77				
	— 2.	29.838	29.412	29.6188	65.1	72.5	54.0	62.30	66.85	60.50	63.22	60.59	3.375	9	1
	— 3.	29.832	29.116	29.7218	62.45	66.5	52.0	58.87	61.45	56.09	58.80			6	4
Aug.	Div. 1.	29.764	29.060	29.4368	61.3	65.75	53.5	57.82	63.82	54.80	58.81				
	— 2.	29.710	29.220	29.4527	61.4	69.5	53.0	58.55	62.95	56.00	59.17	58.95	4.320	9	1
	— 3.	29.920	29.430	29.6640	62.0	65.0	52.0	58.15	62.34	56.06	58.85			11	
Sept.	Div. 1.	29.996	29.222	29.5799	59.3	63.5	47.5	53.65	58.92	51.12	54.23				
	— 2.	29.664	28.834	29.3406	55.9	61.25	47.5	51.65	56.3	49.85	52.60	53.61	3.265	7	3
	— 3.	29.722	29.326	29.5330	57.1	63.5	45.75	52.32	58.57	51.1	54.0			6	4
Oct.	Div. 1.	30.026	29.454	29.6810	56.45	59.75	45.25	50.82	57.3	49.5	52.81				
	— 2.	30.088	29.020	29.5447	54.37	55.0	34.5	46.42	51.1	45.2	47.84	46.18	3.097	10	2
	— 3.	30.088	28.840	29.4912	43.66	47.0	31.5	37.9	43.7	37.8	39.8			5	6
Nov.	Div. 1.	30.270	29.224	29.8077	50.45	55.5	32.5	43.1	51.0	45.3	46.47				
	— 2.	30.203	29.404	29.7576	48.9	51.0	32.0	39.17	44.45	39.4	41.01	40.02	1.245	10	2
	— 3.	29.704	28.658	29.4469	44.0	43.5	24.0	32.05	32.9	32.67	32.64			7	3
Dec.	Div. 1.	29.924	28.610	29.2714	43.6	44.0	30.0	35.17	37.57	35.07	35.77				
	— 2.	29.751	28.563	29.0573	46.7	55.0	30.5	40.32	41.57	42.52	41.13	39.87	1.910	7	3
	— 3.	30.104	29.080	29.7187	49.5	52.5	35.25	42.84	42.95	41.66	42.72			10	1

Mean Temperature of the Year, 48°.04

Total Rain, 25.360|256|109|

1797.

## R E M A R K S.

THERE is little in the meteorological history of this year to be added to what is contained in the table. The winter was very open and mild, with a brisk steady wind, for the most part from W. to S. S. W. There was but little frost, and scarcely any snow.

IN March, the east wind began to prevail; a pretty smart frost was felt in the beginning of the month, and some snow fell on the 6th and 7th; the weather was cold for the season till April, when its temperature came up nearly to the mean.

MAY, except at the beginning, when there were some frosty nights, was favourable, and the wind very often from the S. W. June, on the contrary, was cold, with a great prevalence of N. E. wind.

THOUGH in July and August the weather became much better, yet the general temperature of the summer is by no means high; if we reckon the season of vegetation from the vernal equinox to the 20th of October, we shall find the mean temperature during that time  $53^{\circ}.32$ , which is under the usual quantity.

IN October there was a great fall of rain, with uncommonly high floods. This was preceded by the wind changing on the 19th from the S. W., in which quarter it had been for several weeks, to the N. W., accompanied with frost in the night. On the afternoon of the 20th, the wind came round to N. E. and E., blowing hard, with heavy rain all night. On the 21st, the rivers were much out; the bridge over the Tweed at Kelso was swept away by the flood. The barometer, which had fallen very low on the 17th, continued so till the 26th.

ON the 22d of November, a very sharp frost set in, with the wind N. and N. W. The thermometer, on the 23d at 10 at night, was at  $24^{\circ}$ , and nearly the same again on the 29th.

ON the 4th of December, the wind came to the S. W., which put an end to the frost; and about the middle of the month, the weather became uncommonly fine and warm for the season. The thermometer was for some days constantly above  $50^{\circ}$ , and once as high as  $55^{\circ}$ , and this not at noon, but at night, on the 18th, with a clear sky, and a high wind from S. S. W. The season, however, could not maintain this temperature long; it became colder on the 21st, and the year ended with a slight frost and the wind from the N. W.

METEO-

METEOROLOGICAL TABLE FOR 1798.

		Greatest Height of Bar.	Least Height of Bar.	Mean Height of Bar.	Mean Temp. of Merc. in Bar.	Greatest Height of the Thermometer.	Least Height of Ditto.	Mean Temp. of the Air 8 A. M.	Mean Temp. of Air when warmest.	Mean Temp. of the Air 10 P. M.	Mean Temp. of every ten days.	Mean Temp. of the Month.	Depth of Rain.	Days of West wind.	Days of East wind.
		Inches.	Inches.	Inches.	Inch.	°	°	°	°	°	°	°	Inches.		
Jan.	Div. 1.	30.303	29.003	29.7334	44.3	47.5	28.5	33.25	37.82	33.42	34.83	38.68	2.31c	9	1
	— 2.	29.943	28.785	29.401	46.5	50.5	35.0	39.67	41.65	40.67	40.66			8	2
	— 3.	30.104	28.424	29.4936	48.4	47.75	32.0	40.11	41.5	40.05	40.55			11	0
Feb.	Div. 1.	30.490	29.152	29.968	48.6	50.0	33.75	41.25	44.50	42.65	42.47	39.05	.8cc	9	1
	— 2.	30.024	28.952	29.5955	48.4	51.5	22.25	36.77	41.62	35.55	37.98			9	1
	— 3.	30.043	28.800	29.505	46.1	45.0	30.0	33.84	39.57	36.72	36.71			7	1
March,	Div. 1.	30.023	29.750	29.8338	51.6	54.0	32.0	44.32	49.85	44.12	46.09	41.15	1.56c	7	3
	— 2.	29.990	28.884	29.6801	47.5	45.25	32.75	34.17	41.0	37.07	37.4			4	6
	— 3.	30.101	29.489	29.7886	49.7	50.0	32.50	38.38	44.11	37.38	39.96			5	6
April,	Div. 1.	30.063	28.764	29.4525	50.7	68.5	39.0	47.77	51.11	48.65	49.17	50.51	1.825	10	0
	— 2.	30.010	29.255	29.6735	55.2	69.0	40.0	49.0	56.6	46.9	50.80			4	6
	— 3.	30.062	29.734	29.8794	56.7	63.25	40.5	51.17	55.57	48.0	51.58			3	7
May,	Div. 1.	30.081	29.515	29.8611	57.2	72.0	44.0	49.22	55.4	48.45	51.02	56.21	1.48c	2	8
	— 2.	30.000	29.271	29.6200	58.9	62.5	45.0	51.6	59.1	49.85	58.49			8	2
	— 3.	30.300	29.980	30.1097	62.3	73.0	49.75	58.4	64.77	54.15	59.11			6	5
June,	Div. 1.	30.282	29.461	29.9560	61.5	71.5	50.0	57.9	64.3	55.0	59.1	62.24	1.865	10	0
	— 2.	30.145	29.382	29.8000	64.5	71.25	52.25	61.3	67.3	56.9	61.8			8	2
	— 3.	30.041	29.325	29.7956	63.9	73.0	51.75	61.82	66.75	59.05	65.82			9	1
July,	Div. 1.	29.530	29.162	29.3662	63.6	75.0	55.5	62.42	67.45	58.65	62.84	60.58	2.53c	10	0
	— 2.	29.594	29.088	29.3430	62.6	66.75	53.25	58.52	63.77	55.32	59.2			7	3
	— 3.	29.930	29.380	29.609	62.9	67.25	52.5	59.72	63.10	56.32	59.71			7	4
Aug.	Div. 1.	30.110	29.374	29.7042	64.2	72.25	51.25	60.75	65.02	57.07	60.95	60.05	2.995	8	2
	— 2.	29.884	29.364	29.7452	62.1	66.5	49.5	57.8	61.07	55.97	58.28			6	4
	— 3.	30.174	29.451	29.9366	65.2	74.5	50.25	60.2	66.0	56.60	60.03			9	2
Sept.	Div. 1.	29.804	29.062	29.4708	63.0	70.0	49.25	59.1	63.45	54.37	58.97	55.91	2.515	6	4
	— 2.	29.888	28.722	29.5516	63.4	63.0	47.75	56.1	60.72	53.77	56.86			5	5
	— 3.	29.728	28.924	29.3570	60.2	65.0	43.0	51.77	55.2	48.72	51.80			8	2
Oct.	Div. 1.	30.110	29.216	29.8207	58.3	62.5	46.75	55.05	58.07	52.32	55.14	48.69	2.105	7	3
	— 2.	30.010	29.162	29.4818	56.6	59.5	36.5	46.17	51.02	46.55	47.91			8	2
	— 3.	29.611	28.975	29.3176	56.7	52.5	41.0	44.9	48.9	46.02	43.04			7	4
Nov.	Div. 1.	29.780	28.588	28.9600	57.0	55.5	35.5	43.52	47.77	43.65	44.98	41.39	2.21c	8	2
	— 2.	30.052	29.438	29.7588	52.5	43.0	29.5	37.02	41.3	37.12	39.76			5	5
	— 3.	29.871	28.394	29.2345	52.1	51.5	28.0	37.8	40.97	39.6	39.45			7	3
Dec.	Div. 1.	30.225	28.950	29.6136	53.3	44.5	35.0	37.75	41.4	38.92	39.35	36.84	1.66c	6	4
	— 2.	30.248	29.050	29.5490	51.9	46.3	28.5	38.92	39.77	38.37	39.02			1	9
	— 3.	30.428	29.408	30.022	48.0	44.0	18.0	32.04	34.04	32.45	32.17			6	5

Total Rain, 23.855250115

Mean Temperature of the Year, 49°.28

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1798.

## R E M A R K S.

THE climate of this part of our Island hardly admits of a finer season than that of 1798. The winter, though not altogether without frost, was on the whole mild and open. In March, indeed, the temperature was rather below the mean; but in April it rose considerably above it, and continued so till the end of October.

IT is remarkable of this year, that the temperature changed much more regularly than usual, increasing gradually till the end of June, and diminishing in the same manner till the end of December, without any considerable retrogradation. The mean temperature is more than  $1^{\circ}.5$  above that of an ordinary year; and this difference fell chiefly on the summer months. The mean temperature of June, July and August, was  $61^{\circ}$ ; the thermometer was often above 70, and never so low as 49 degrees. The vegetating season, reckoned from the 20th of March to the 20th of October, has for its mean temperature  $56^{\circ}.17$ , considerably above the mean of an ordinary year; and, as along with this the rains fell seasonably, the crops of all kinds were very abundant. The fine weather of this year extended over the whole of Britain, and indeed over the greatest part of Europe.

METEO-

METEOROLOGICAL TABLE FOR 1799.

		Greatest Height of Barometer.	Least Height of Bar.	Mean Height of Bar.	Mean Temperature of Merc. in Barometer.	Greatest Height of the Thermometer.	Least height of Ditto.	Mean Temp. of the Air 8 A. M.	Mean Temp. of Air when warmest.	Mean Temp. of the Air 10 P. M.	Mean Temp. in every ten days.	Mean Temp. of the Month.	Depth of Rain.	Days of West wind.	Days of East wind.
		Inches.	Inches.	Inches.	Inch.	°	°	°	°	°	°	°	Inches.		
Jan.	Div. 1.	30.301	29.744	30.0489	51.6	47.0	28.0	35.32	38.42	36.62	36.78			8	2
	— 2.	29.940	29.046	29.6598	54.5	49.0	38.5	43.27	45.30	44.10	44.18	37.61	.335	10	
	— 3.	29.656	28.854	29.2906	50.1	43.0	25.5	30.30	33.05	32.57	31.9			8	3
Feb.	Div. 1.	29.858	29.251	29.5582	45.3	40.0	21.25	27.32	31.97	27.10	28.8			2	8
	— 2.	29.769	28.826	29.2571	52.0	45.5	28.0	35.97	40.09	37.47	38.8	37.60	.964	5	5
	— 3.	29.814	28.685	29.4205	56.5	57.75	37.5	43.40	48.53	43.60	45.18			8	
March,	Div. 1.	30.148	29.500	29.9034	56.0	54.75	35.5	39.55	43.62	37.45	40.20			3	7
	— 2.	30.100	29.280	29.5975	55.3	45.0	32.25	36.57	41.08	34.97	37.78	38.80	.695	5	5
	— 3.	30.050	29.122	29.5236	52.8	49.0	31.25	37.70	40.09	36.13	38.42			5	11
April,	Div. 1.	29.860	29.072	29.4586	49.7	42.5	31.0	34.70	38.67	34.12	35.83			1	9
	— 2.	30.080	28.666	29.2674	54.4	50.0	37.5	42.67	47.12	42.02	43.90	41.91	2.280	6	4
	— 3.	29.986	29.170	29.7251	57.0	63.5	36.75	45.32	50.50	42.20	46.0			6	0
May,	Div. 1.	29.728	29.312	29.5605	55.7	52.0	34.5	42.57	46.62	40.60	43.08			2	8
	— 2.	30.182	29.162	29.6807	57.1	61.50	37.5	44.57	50.45	44.05	46.02	46.88	3.355	5	5
	— 3.	30.000	29.185	29.6777	60.5	66.50	44.0	52.40	58.00	49.25	51.55			11	0
June,	Div. 1.	30.164	28.585	29.6402	61.1	73.75	43.0	56.25	60.37	50.72	55.80			6	4
	— 2.	30.260	29.560	29.9839	51.8	66.50	46.5	52.85	58.40	51.02	54.09	56.36	1.335	2	8
	— 3.	30.100	29.600	29.8625	62.5	76.50	49.0	57.12	65.17	55.32	59.20			6	4
July,	Div. 1.	29.764	29.214	29.7151	62.3	70.5	53.0	60.85	65.65	56.90	61.13			8	2
	— 2.	29.676	29.068	29.3556	60.6	66.0	50.0	55.62	60.62	53.45	56.56	58.09	3.125	5	5
	— 3.	29.776	29.280	29.5629	59.2	67.5	50.75	55.80	59.68	54.34	56.60			4	7
Aug.	Div. 1.	29.884	29.024	29.4773	60.5	66.5	50.5	58.22	59.45	53.90	57.19			3	7
	— 2.	29.664	28.760	29.2750	60.5	64.5	50.5	56.45	60.62	53.17	56.74	56.81	4.450	6	4
	— 3.	29.820	29.110	29.5112	59.8	62.0	49.0	55.63	60.20	53.70	56.51			11	0
Sept.	Div. 1.	30.164	29.860	30.0120	62.5	66.25	51.5	55.50	62.00	54.52	57.34			6	4
	— 2.	29.862	28.850	29.3300	60.3	64.0	50.0	53.32	57.17	52.82	54.44	54.26	4.085	3	7
	— 3.	29.720	28.912	29.2634	58.0	57.5	44.5	50.47	54.62	48.97	51.02			8	2
Oct.	Div. 1.	29.640	29.010	29.3590	56.0	56.25	38.5	46.97	51.80	45.25	48.01			8	2
	— 2.	29.786	29.210	29.5028	56.4	56.75	35.0	44.17	52.03	45.75	47.32	47.01	2.035	6	4
	— 3.	30.012	29.012	29.5438	52.6	55.5	35.5	44.40	48.68	44.1	45.72			5	6
Nov.	Div. 1.	29.542	28.525	29.0798	56.5	52.5	33.0	42.92	46.17	41.0	43.36			9	1
	— 2.	30.200	28.923	29.4466	56.5	49.75	34.0	39.80	44.75	39.52	41.02	41.70	1.645	10	0
	— 3.	30.252	29.464	29.8733	54.9	51.5	33.0	38.72	42.27	41.17	40.72			7	3
Dec.	Div. 1.	30.024	28.950	29.5164	50.0	48.5	35.5	40.47	42.27	39.50	40.74			2	8
	— 2.	30.370	29.610	29.9307	51.9	42.5	28.0	35.32	37.37	36.20	36.29	36.52	1.370	8	2
	— 3.	30.352	29.792	30.1114	49.7	38.0	18.0	30.61	36.20	30.80	32.53			8	3

Total Rain, 25.874/211/154

Mean Temperature of the Year, 46°.13

1799.

## R E M A R K S.

THE year 1799, though it afterwards proved so unfavourable, began with very mild weather. January was uncommonly pleasant, and the wind generally from the S. W.

IN the beginning of February, the wind changed to N. E.; on the 7th, the thermometer stood at  $22^{\circ}$ ; and the morning of the 9th was remarkable for the most violent storm of wind and snow that is remembered in this country: the wind was a little south of east, and the thermometer as low as  $25^{\circ}$ . The same day afforded a striking example of the vicissitudes in our climate; for the storm, which began about one in the morning, having continued till noon, the wind soon after came to the S. W.; a thaw began immediately, and in the evening the thermometer stood at  $40^{\circ}$ . The frost, however, soon returned, but without severity; the weather in the latter end of the month was mild, though the snow, which had been much drifted on the 9th, still lay deep in many places.

ON the 3d of March, the wind came to the N. E., and the series of cold and disagreeable weather began, which extended over almost all the remainder of the year. The winds, during the whole of March, were dry and parching; the nights frosty, and the mean temperature hardly above that of February.

APRIL was equally unfavourable; it snowed frequently, and the wind, though often in the west, was always so far to the north, that it was never accompanied with warmth. Even at the end of the month there were scarce any signs of vegetation, and the trees seemed hardly farther advanced than in February.

THROUGH the whole of May the season continued extremely backward; the wind, whether in the east or the west, was always far to the north, and the medium temperature of the month about  $3^{\circ}$  lower than in ordinary years.

JUNE was a little more favourable; and indeed the only part of the season that afforded any thing like good summer weather, was the last ten days of June, and the first ten of July. The mean temperature, during that time, was about  $60^{\circ}$ , and was not so high, by several degrees, for any other period of the same length in the whole summer.

FROM this time the weather became continually colder. There was little sunshine, and a great deal of rain, during the remaining part of July, and the whole of August and September.

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THE month of October was in itself tolerable; and nearly of the usual temperature; but by no means able to make up the deficiencies of the former season. The harvest was not generally got in till the end of November, and, in high grounds, hardly till the end of December.

ONE of the peculiarities of this season was, that the east wind continued to blow very frequently after the solstice, through the whole summer; and always from a northern point. The west winds, also, were generally far to the north, which seldom happens in this climate, from the time of the summer solstice to the setting in of frost in November or the latter end of October.

THE general impression which this year has left on every body's mind is, that it was very rainy; and yet it does not appear, from the register, that more rain fell in 1799 than in ordinary years. The fact is, that the only rainy months were July, August and September, particularly the two latter; in these three the depth of rain was nearly a foot, which is at least double of the usual quantity. This quantity, also, fell not in heavy showers, with intervals of sunshine, as is usual in summer, but in drizzling, long continued rains, with foggy weather, as in winter. Hence the amount of it was very naturally over-rated.

THE mean temperature of the whole year is  $46^{\circ}.18$ , more than  $1^{\circ}.5$  below the usual mean. But the mean temperature of the season of vegetation, computed from the 20th of March to the 20th of October, is no more than  $51^{\circ}.27$ , almost  $5^{\circ}$  below that of 1798. This deficiency of temperature may appear at first sight hardly adequate to that deficiency in the crop which is ascribed to it; but it should be considered, that vegetation scarcely proceeds at all with a temperature under  $40^{\circ}$ , so that this may not improperly be regarded as the point of heat at which vegetation begins, and the boundary, in as much at least, as respects agriculture, between fruitfulness and sterility. Now,  $56^{\circ}$  is the mean temperature of a good season, in this country, as we know from the instance of 1798, and therefore  $16^{\circ}$  of heat is the whole distance between the mere germination of vegetables and the fullest maturity they can attain in our climate. A deficiency of  $5^{\circ}$ , therefore, which is nearly a third of the whole  $16^{\circ}$ , must necessarily be accompanied with a great shortcoming in the maturity of all vegetable productions.

WHETHER the quantity of the crop may be expected to be proportional to the excess of the mean temperature of the vegetating season above  $40^{\circ}$ , so that the crop of this year should be to that of the preceding as two to three, or if it must be in a greater or a less ratio, may deserve to be more accurately considered. There is, however, reason to think, that the variations of the crop, at least of the corn crop, will be greater than in proportion to the variations of temperature; for, if the mean heat of the vegetating season were to fall as much below that of 1799, as the heat of 1799 did below that of 1798, it would be reduced to  $46^{\circ}$ , a temperature so low, as would certainly prevent the ripening of corn altogether.

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By doubling the deficiency of the heat, therefore, we do a great deal more than double the deficiency of the crop, so that the latter varies in a higher ratio than the former.

THE limit at which corn will not ripen, is probably higher than  $46^{\circ}$ , and may perhaps be stated at  $48^{\circ}$ . If it be alarming to think that our climate has, in any case, come so near to a line, beyond which the labours of the husbandman are ineffectual, it is comfortable to reflect, that the resources of the country have been able to provide a remedy even for so great an evil, and to avert all its most fatal effects.

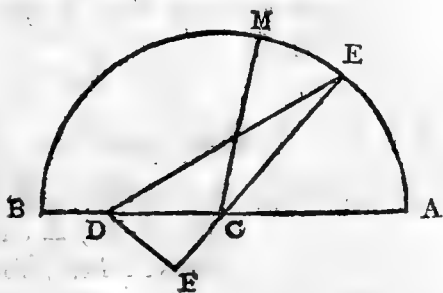
IX.

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IX. *A NEW and UNIVERSAL SOLUTION of KEPLER'S PROBLEM. By JAMES IVORY, Esq. Communicated by JOHN PLATFAIR, Professor of Mathematics and F. R. S. Edin.*

[Read July 7. 1800.]

1. **K**EPLER, having discovered the laws that regulate the motion of a planet in its orbit, proposed the following problem, for determining the true place of a planet at any given time: "To draw a straight line DE, from an eccentric point D in the diameter of a semicircle AEB, so that the whole semicircle may be to the sector ADE, in a given ratio."



IN resolving this problem, we are to take the quadrature of the circle for granted: and therefore, if C be the centre of the circle, and if the sector AGM be taken, such, that the whole semicircle is to the sector ACM in the required given ratio, the problem may be otherwise enunciated: "To draw a straight line DE from an eccentric point D, to cut off a sector ADE, that shall be equal to the given sector ACM."

THE given arch AM, or the given angle ACM, is usually called the mean anomaly; and the arch AE, or the angle ACE, the anomaly of the eccentric: the problem, therefore, is reduced

ced to this: "The mean anomaly being given, to find the anomaly of the eccentric."

2. DRAW the straight line DF at right angles to the diameter passing through the point E: Then, since the sector ADE is equal to the sector ACM, and the space ACE is common to both, the triangle EDC will be equal to the circular sector ECM: therefore, it is manifest, that the straight line DF is equal to the circular arch EM.

SUPPOSE that the radius of the circle is unity; and let  $m = \text{arch AM}$ ,  $\mu = \text{arch AE}$ , and  $\epsilon = \text{eccentricity DC}$ : Then, since  $DF = \epsilon \sin \mu$ , we shall have this equation, expressing the relation between the arch of mean anomaly and the arch of eccentric anomaly:

$$m - \mu = \epsilon \sin \mu.$$

3. IN the equation just found, let us put  $m = 2n$  and  $\mu = 2\nu$ : and, remarking that  $\sin \mu = \sin 2\nu = 2 \sin \nu \times \cos \nu$ , we shall readily obtain,

$$n - \nu = \epsilon \sin \nu \times \cos \nu:$$

And, if we further suppose  $e = \epsilon \times \frac{\sin (n - \nu)}{n - \nu}$ , and, by means of this formula, exterminate  $\epsilon$ , we shall find

$$\sin (n - \nu) = e \sin \nu \times \cos \nu.$$

It may be remarked here, for the greater precision, that, from the nature of the problem, the arches,  $m$  and  $\mu$ , never exceed  $180^\circ$ ; and, of consequence, the arches  $n$  and  $\nu$ , never exceed  $90^\circ$ .

4. IF we consider  $e$  as a known or given quantity, it is evident, that the equation last found will no longer have the form of a transcendental equation; and that the arch  $\nu$  will be determined, when the arch  $n$  is given, by a finite equation, resolvable by known methods.

IN strictness, indeed, we cannot consider  $e$  as a given quantity; for the exact value of  $e$  depends upon the arch  $\nu$ , and cannot be known unless the length of that arch were known;  
in

in other words, unless the problem, of which we are treating, were already resolved. But it is easy to demonstrate, that  $e$  is always very nearly equal to the eccentricity  $\varepsilon$ ; and that, therefore, we may assume  $e = \varepsilon$ , at least for a first approximation to the value of  $\nu$ .

For it is clear, from the most elementary principles, that the maximum value of  $\sin \nu \times \cos \nu$  is equal to  $\frac{1}{2}$ : therefore the arch  $n - \nu$ , determined by the equation  $\sin (n - \nu) = \varepsilon \sin \nu \times \cos \nu$ , when greatest of all, can never exceed  $\frac{\varepsilon}{2}$ . It is also evident, from the nature of KEPLER'S problem, that  $\varepsilon$  can never be greater than unity; because the point D is supposed to be always taken in the diameter, and never without the circle. Therefore, even in the extreme case, when  $\varepsilon = 1$ , the arch  $n - \nu$  can never be greater than  $\frac{1}{2}$ .

BUT small arches of a circle are very nearly equal to their sines; a proposition that we may extend, without great error, to all arches not exceeding  $30^\circ$ . Now, we have shewn, that the length of the arch  $n - \nu$  can never exceed  $\frac{1}{2}$ , and therefore, that arch will always be less than the arch of  $30^\circ$ , the sine of which is  $\frac{1}{2}$ . Therefore, the fraction  $\frac{\sin (n - \nu)}{n - \nu}$  will always be very nearly equal to unity; and, consequently, the value of  $e$  determined by the formula  $e = \varepsilon \times \frac{\sin (n - \nu)}{n - \nu}$ , will, in all cases whatsoever, differ but little from  $\varepsilon$ .

ASSUMING, therefore,  $e = \varepsilon$ , if we denote by  $\pi$  the value of  $\nu$  corresponding to this value of  $e$ , in the equation  $\sin (n - \nu) = e \sin \nu \times \cos \nu$ ; we may consider  $\pi$  as a first approximation to half the arch of eccentric anomaly.

5. HAVING thus found one approximate value of  $\nu$ , it is easy to find as many others as we please, by means of the formulas already investigated.

For, in the formula  $e = \varepsilon \times \frac{\sin(n - v)}{n - v}$ , let  $\pi$ , the first approximate value already found, be substituted for  $v$ , and let  $\varepsilon'$  denote the value of  $e$  that will result from the substitution; or, which is the same thing, let  $\varepsilon' = \varepsilon \times \frac{\sin(n - \pi)}{n - \pi}$ : And further, let  $\varepsilon'$  be substituted for  $e$  in the equation  $\sin(n - v) = e \sin v \times \cos v$ , and let  $\pi'$  denote the corresponding value of  $v$ : then will  $\pi'$  be a second approximation to the arch  $v$ .

IN like manner,  $\pi'$  being substituted for  $v$  in the formula  $e = \varepsilon \times \frac{\sin(n - v)}{n - v}$ , will give a new value of  $e$ , denoted by  $\varepsilon''$ : and, by means of the equation  $\sin(n - v) = e \times \sin v \times \cos v$ , this new value of  $e$  will give a third approximation to the arch  $v$ , denoted by  $\pi''$ . And it is manifest, that the series of arches,  $\pi, \pi', \pi'', \&c.$  approximating to the value of  $v$ , may be continued indefinitely.

6. I now say, that the arches  $\pi, \pi', \pi'', \&c.$  which constitute the series of approximations to the value of  $v$ , are alternately too small and too great: that is, the first, third, fifth,  $\&c.$  terms in the series are all less; but the second, fourth, sixth,  $\&c.$  terms in the series are all greater, than the exact value of  $v$ .

For, if, in the equation  $\sin(n - v) = e \times \sin v \times \cos v$ , we write  $\sin n \cos v - \cos n \sin v$ , for  $\sin(n - v)$ , and divide both sides by  $\sin v \times \cos v$ , we shall get

$$e = \frac{\sin n}{\sin v} - \frac{\cos v}{\cos v}:$$

in this formula  $e$  vanishes when  $n = v$ : and, supposing the arch  $v$  to decrease, it is manifest that the positive part,  $\frac{\sin n}{\sin v}$ , will increase, and that the negative part,  $\frac{\cos n}{\cos v}$ , will decrease: therefore  $e$  will increase when  $v$  decreases; and the less the arch  $v$  is, the greater will be the value of  $e$ . This, it is evident, must also be true, when taken inversely; that is, the greater  $e$  is, the less will be the arch  $v$ .

LET

LET us now consider  $\pi$ , the first term in the series of approximate arches; this arch is the value of  $v$  in the equation  $\sin(n - v) = e \times \sin v \times \cos v$ , when  $\varepsilon$  is substituted for  $e$ ; but, since we have  $e = \varepsilon \times \frac{\sin(n - v)}{n - v}$ , it is evident, that  $e$  is less than  $\varepsilon$ , and consequently  $v$  will be greater than  $\pi$ .

AGAIN, take  $\pi'$ , the second term in the series of approximate arches. This arch is the value of  $v$  in the equation  $\sin(n - v) = e \times \sin v \times \cos v$ , when  $\varepsilon$  is substituted for  $e$ : Now,  $\varepsilon = \varepsilon \times \frac{\sin(n - \pi)}{n - \pi}$ , and  $e = \varepsilon \times \frac{\sin(n - v)}{n - v}$ ; and since  $v$  has been shewn to be greater than  $\pi$ , it is evident that  $n - \pi$  will be greater than  $n - v$ : but the greater arch has to its sine the greater ratio; therefore the fraction  $\frac{\sin(n - \pi)}{n - \pi}$ , will be less than the fraction  $\frac{\sin(n - v)}{n - v}$ : consequently  $\varepsilon$  will be less than  $e$ ; and therefore  $\pi'$  will be greater than  $v$ .

AND, in general, if, in the formula  $e = \varepsilon \times \frac{\sin(n - v)}{n - v}$ , we substitute a greater arch for  $v$ , we shall have a value of  $e$  greater than its true value; but, if we substitute a less arch for  $v$ , we shall have a value of  $e$  less than its true value: but we have demonstrated, that, in the equation  $\sin(n - v) = e \sin v \times \cos v$ , the greater  $e$  is, the less will the arch  $v$  be: from which considerations the truth of what we have asserted above is evident, viz. that the arches  $\pi, \pi', \pi'', \&c.$  continued indefinitely, are alternately too small and too great.

7. LET us now compare together the alternate terms in the series of approximate arches; and it will not be difficult to perceive, that the first, third, fifth, &c. terms, which have been shewn to be all less than half the arch of eccentric anomaly, continually increase; but that the second, fourth, sixth, &c. terms, which have been shewn to be all greater than half the arch of eccentric anomaly, continually decrease.

FOR, it is obvious, that the greatest value of  $e$ , in the formula,  $e = \varepsilon \times \frac{\sin(n - \nu)}{n - \nu}$ , is when  $n - \nu = 0$ , in which case  $e = \varepsilon$ : therefore, the arch  $\pi$  will be the least of all the approximate arches,  $\pi, \pi', \pi'', \&c.$ : therefore  $\pi$  is less than  $\pi''$ , the third term in the series. Again, we have  $\varepsilon' = \varepsilon \times \frac{\sin(n - \pi)}{n - \pi}$ , and  $\varepsilon'' = \varepsilon \times \frac{\sin(n - \pi'')}{n - \pi''}$ : and since  $\pi$  is less than  $\pi''$ , it is manifest that  $n - \pi$  will be greater than  $n - \pi''$ : consequently  $\varepsilon'$  will be less than  $\varepsilon''$ ; whence it follows, that  $\pi'$ , the value of  $\nu$  corresponding to  $\varepsilon'$ , will be greater than  $\pi''$ , the value of  $\nu$  corresponding to  $\varepsilon''$ . Thus, then, we have shewn, that the first term in the series  $\pi, \pi', \pi'', \pi''', \&c.$  is less than the third term; but that the second term is greater than the fourth: and, it is clear that, by a similar mode of reasoning, we may prove that the third term is less than the fifth; but the fourth term greater than the sixth; and so on.

8. It has now been demonstrated, that the arches in the series  $\pi, \pi', \pi'', \&c.$  are alternately less and greater than half the arch of eccentric anomaly; and further, that the terms in the series, less than that arch, continually increase, while the terms in the series, greater than that arch, continually decrease; whence, it is manifest, that by computing more and more terms of the series, we shall have the value of the arch of eccentric anomaly within narrower and narrower limits. The arches  $\pi, \pi', \pi'', \&c.$  form a series of approximations, converging to the true length of half the arch of eccentric anomaly, and erring alternately in defect and in excess.

AND here a question occurs. It may be asked, Do the terms of the series  $\pi, \pi', \pi'', \&c.$  converge slowly to the arch sought? or, Do they converge rapidly to it? According to the answer that we shall be able to give to this question, our method of solution is to be reckoned more or less perfect and valuable.

WE



WE have hitherto considered the series of approximations to the value of  $\nu$  to begin with the arch  $\pi$ ; but, in effect, the series may be considered to begin with the arch  $n$ . For, if in the formula  $e = \epsilon \times \frac{\sin(n - \nu)}{n - \nu}$ , we substitute  $n$  for  $\nu$ , the resulting value of  $e$  will be  $\epsilon$ , to which the arch  $\pi$  corresponds in the equation  $\sin(n - \nu) = e \times \sin \nu \times \cos \nu$ . It is clear, therefore, that the arch  $\pi$  is derived from the arch  $n$ , precisely in the same way that  $\pi'$  is derived from  $\pi$ ; or that any other term in the series is derived from the term that immediately precedes it.

THE error of the arch  $n$ , considered as an approximation to  $\nu$ , is  $n - \nu$ : taking the extreme case, when  $\epsilon = 1$ , (in which case the convergency of the series is evidently the slowest), the length of the arch  $n - \nu$ , when a maximum, is (Art. 3.) equal to  $\frac{1}{2}$ , corresponding to  $28^\circ 39'$  nearly. Therefore the arch  $n$ , considered as an approximation to  $\nu$ , is very wide of the truth: And, if we can prove that the error of  $\pi$ , the second term in the series, is nevertheless inconsiderable, we shall be entitled to conclude favourably with regard to the convergency of the series.

THE error of the arch  $\pi$  is  $\nu - \pi$ , and we are now to inquire, to what degree of magnitude this arch may attain. For this purpose let us consider the two equations,

$$(n - \nu) = \epsilon \times \sin \nu \times \cos \nu,$$

$$\sin(n - \pi) = \epsilon \times \sin \pi \times \cos \pi,$$

by means of which the arches  $\nu$  and  $\pi$  are determined when the arch  $n$  is given. It is clear, that the quantities  $\epsilon \times \sin \nu \times \cos \nu$  and  $\epsilon \times \sin \pi \times \cos \pi$  are evanescent, when  $\nu = 0$  and  $\pi = 0$ , and also when  $\nu = 90^\circ$  and  $\pi = 90^\circ$ : therefore we shall have  $n = \nu = \pi$ , not only when  $n = 0$ , but also when  $n = 90^\circ$ . It has also been shewn, that the arch  $\nu$  is greater than the arch  $\pi$ : therefore the quantity  $\nu - \pi$  vanishes, when  $n = 0$  and when  $n = 90^\circ$ , and between these two limits it is always positive; consequently there is an intermediate value of  $n$ , where the arch  $\nu - \pi$  will be a maximum.

SINCE

SINCE  $2 \sin \nu \times \cos \nu = \sin 2\nu$ , and  $2 \sin \pi \times \cos \pi = \sin 2\pi$ , the two equations become,

$$n - \nu = \frac{\varepsilon}{2} \sin 2\nu,$$

$$\sin (n - \pi) = \frac{\varepsilon}{2} \sin 2\pi;$$

take the fluxions of these equations, making  $n$ ,  $\nu$ , and  $\pi$  variable; then, having brought  $\dot{n}$  to stand by itself on one side, we shall find,

$$\dot{n} = \dot{\nu} (1 + \varepsilon \cos 2\nu),$$

$$\dot{n} = \dot{\pi} \left( 1 + \frac{\varepsilon \cos 2\pi}{\cos (n - \pi)} \right).$$

whence, by equating these two values of  $\dot{n}$ ,

$$\dot{\nu} (1 + \varepsilon \cos 2\nu) = \dot{\pi} \left( 1 + \frac{\varepsilon \cos 2\pi}{\cos (n - \pi)} \right).$$

Now  $\nu - \pi$  is a maximum when  $\dot{\nu} - \dot{\pi} = 0$ , that is, when  $\dot{\nu} = \dot{\pi}$ : therefore, if we divide both sides of the preceding equation by the equal quantities  $\dot{\nu}$  and  $\dot{\pi}$ ; and further, reject what is common to both sides, and divide the remainders by  $\varepsilon$ , we shall have, in the case when  $\nu - \pi$  is a maximum,

$$\cos 2\nu = \frac{\cos 2\pi}{\cos (n - \pi)}.$$

If we combine this equation with the two equations that express, in general, the relations of  $n$  to  $\nu$ , and of  $n$  to  $\pi$ , we shall have three equations sufficient to determine the three arches,  $n$ ,  $\nu$  and  $\pi$ , in the case when  $\nu - \pi$  is a maximum. But, as one of the equations is transcendental, this could only be done by the method of infinite series, and would lead into very perplexed calculations. We may, however, by an easy formula, determine a limit, which the quantity  $\nu - \pi$ , when greatest of all, cannot exceed: this will be sufficient for the purpose we have at present in view.

FROM the equation  $\text{cof } 2\nu = \frac{\text{cof } 2\pi}{\text{cof } (n - \pi)}$ , we readily obtain

$$\sin 2\nu = \frac{\sqrt{(\text{cof}^2 (n - \pi) - \text{cof}^2 2\pi)}}{\text{cof } (n - \pi)} = \frac{\sqrt{\sin^2 2\pi - \sin^2 (n - \pi)}}{\text{cof } (n - \pi)};$$

but  $\sin (n - \pi) = \frac{\varepsilon}{2} \sin 2\pi$ , therefore, by substitution,

$$\sin 2\nu = \frac{\sin 2\pi}{\text{cof } (n - \pi)} \times \sqrt{1 - \frac{\varepsilon^2}{4}}.$$

Multiply both sides by  $\frac{\varepsilon}{2}$ ; and substitute, for  $\frac{\varepsilon}{2} \sin 2\nu$ , its equal  $n - \nu$ ; and for  $\frac{\varepsilon}{2} \sin 2\pi$ , its equal  $\sin (n - \pi)$ , and we shall have,

$$n - \nu = \frac{\sin (n - \pi)}{\text{cof } (n - \pi)} \times \sqrt{1 - \frac{\varepsilon^2}{4}}.$$

Subtract both sides of this equation from  $n - \pi$ , and, remarking that  $\tan (n - \pi) = \frac{\sin (n - \pi)}{\text{cof } (n - \pi)}$ , there will result,

$$\nu - \pi = n - \pi - \tan (n - \pi) \times \sqrt{1 - \frac{\varepsilon^2}{4}}.$$

To simplify this formula, I put  $n - \pi = g$ , and  $a = \sqrt{1 - \frac{\varepsilon^2}{4}}$ : and so, (in the case when  $\nu - \pi$  is a maximum) we have

$$\nu - \pi = g - a \tan g.$$

LET us now consider the function  $g - a \tan g$ : because  $a = \sqrt{1 - \frac{\varepsilon^2}{4}}$  is less than unity, it is evident that we may take the arch  $g$  so small, that  $a \tan g$  shall be less than  $g$ ; and, that there is a value of  $g$  such that  $g = a \tan g$ : it is also manifest, that between the limits  $g = 0$  and  $g = a \tan g$ , (in both which cases  $g - a \tan g = 0$ ) the function  $g - a \tan g$  attains a maximum value.

IF, therefore, we can prove, that the arch  $n - \pi$  is always between the limits  $g = 0$  and  $g = a \tan g$ ; it will follow that the maximum value of the function  $g - a \tan g$  is greater than the arch  $\nu - \pi$ , even when that arch is greatest of all.

Now,

Now, since  $\sin(n - \pi) = \varepsilon \sin \pi \times \cos \pi$ , it is evident that  $\sin(n - \pi)$  can never be greater than  $\frac{\varepsilon}{2}$ . Also, if  $\varphi = a \tan \varphi$ , we shall have  $\sin \varphi < a \tan \varphi$ ; therefore  $\frac{\sin \varphi}{\tan \varphi} = \cos \varphi < a$ : therefore, since  $a = \sqrt{1 - \frac{\varepsilon^2}{4}}$ , it is manifest that  $\sin \varphi$  is greater than  $\frac{\varepsilon}{2}$ . Whence it is obvious, that  $n - \pi$  is less than the arch  $\varphi$ , determined by the formula  $\varphi = a \tan \varphi$ .

It now remains that we determine the maximum value of the function  $\varphi - a \tan \varphi$ : for this purpose, let  $y = \tan \varphi$ , and since  $\varphi = \frac{y}{1+y^2}$ , we shall have, by the usual method,

$$\frac{1}{1+y^2} - a = 0;$$

whence  $y = \tan \varphi = \sqrt{\left(\frac{1}{a} - 1\right)}$ .

THEREFORE, if we take  $\tan \varphi = \sqrt{\frac{1}{\sqrt{1-\frac{\varepsilon^2}{4}}} - 1}$ , the arch  $n - \pi$  can never be greater than  $\varphi - a \tan \varphi$ .

If we take  $\varepsilon = 1$ , we shall have  $\tan \varphi = \sqrt{\left(\frac{2}{\sqrt{3}} - 1\right)}$ , whence  $\varphi = 21^\circ 28' 14''$  nearly, and  $\varphi - \frac{\sqrt{3}}{2} \tan \varphi = .03411$ , which is the length of an arch of  $1^\circ 57'$  nearly. It is therefore certain, that, even in the extreme case, when  $\varepsilon = 1$ , the arch  $\pi$  cannot differ from half the arch of eccentric anomaly, more than  $1^\circ 57'$ ; a very small error, considering that the first supposition of  $n = \nu$  is very wide of the truth. We may therefore conclude, that the series  $\pi, \pi', \pi'', \&c.$  converges to the true length of half the arch of eccentric anomaly with uncommon rapidity.

9. WE have now shewn, that, by means of the finite equation  $\sin(n - \nu) = \varepsilon \sin \nu \times \cos \nu$ , together with the formula  $\varepsilon = \varepsilon \times \frac{\sin(n - \nu)}{n - \nu}$ , we may deduce a series of arches, converging very rapidly to half the arch of eccentric anomaly. The reasonings

sonings that have led us to this conclusion are quite general, and hold good in every state of the data of the problem. To have a complete and universal solution of this famous problem, it only remains, that we investigate a rule for computing the arch  $\nu$ , from the equation  $\sin(n - \nu) = e \times \sin \nu \times \cos \nu$ , supposing  $n$  and  $e$  to be given quantities. This is what we are now to set about.

THE given equation,  $\sin(n - \nu) = e \times \sin \nu \times \cos \nu$ , is easily transformed into this, (Art. 6.)

$$e = \frac{\sin n}{\sin \nu} - \frac{\cos n}{\cos \nu}.$$

Let ADB be a semicircle, and let the diameter DC be drawn at right angles to AB: Take the arch AM =  $n$ , and in CM produced take CF =

$\frac{1}{e} \times CA$ : From the point F draw the straight line FGH, so that the part GH, intercepted between AB and CD, in the angle DCB, may be equal to

CA, the radius of the circle: and, lastly, through C draw CN, parallel to FH: I say, that the arch AN is equal to  $\nu$ .

FROM the two triangles FCH, FCG, we have

$$\sin FHA : \sin FCA :: FC : FH = FC \times \frac{\sin AM}{\sin AN},$$

$$\sin FGD : \sin FCD :: FC : FG = FC \times \frac{\cos AM}{\cos AN}.$$

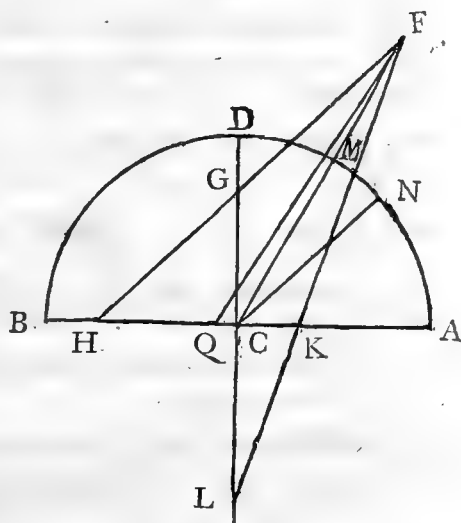
Now, FH - FG = HG = CA, therefore

$$CA = FC \times \left( \frac{\sin AM}{\sin AN} - \frac{\cos AM}{\cos AN} \right);$$

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consequently,



consequently, since  $\frac{CA}{CF} = e$ , we get

$$e = \frac{\sin AM}{\sin AN} = \frac{\cos AM}{\cos AN};$$

therefore, because  $AM = n$ , it is manifest that  $AN = v$ .

Thus, then, the resolution of the equation  $\sin(n - v) = e \sin n \times \cos v$ , coincides with that case of the general problem, "*De inclinationibus*" of the ancients, in which the two given lines are supposed to be straight lines, intersecting one another

at right angles. If we take  $CF = \frac{1}{\varepsilon} \times CA$ , the arch  $AN$ , found by the construction above, will be no other than the arch  $\pi$ , the first term in the series that we have already discussed: and, in like manner, if we take  $CF$  successively equal to  $\frac{1}{\varepsilon} \times \frac{(n - \pi)}{\sin(n - \pi)} \times CA$ ;

$$\frac{1}{\varepsilon} \times \frac{(n - \pi')}{\sin(n - \pi')} \times CA; \quad \frac{1}{\varepsilon} \times \frac{(n - \pi'')}{\sin(n - \pi'')} \times CA; \quad \text{and so on:}$$

we may find, by the same construction, the other terms  $\pi'$ ,  $\pi''$ ,  $\pi'''$ , &c. of that series.

If, therefore, we are to rest satisfied with a geometrical construction, we may consider KEPLER's problem as already resolved. For, it is manifest, from what has been proved, that, by means of the known and elementary problem, "*De inclinationibus*," we may, in all cases, approximate to the arch of eccentric anomaly as nearly as may be required. It must be confessed, however, that a construction of this kind, let it be ever so ingenious or elegant, is of no use to the astronomer, who seeks for a rule by which to conduct his calculations, and who will not be satisfied with a speculation of the mind.

10. The problem, "*De inclinationibus*," when the two lines given by position are supposed to be straight lines, is, in general, a solid problem. The geometrical construction cannot be effected, unless by the help of the conic sections; and the solution, by the modern algebra, leads to an equation of the fourth power,

power, or at least to an equation of the third power: It is known, however, that in one particular situation of the given point, the problem becomes more simple. This happens when the given point is situated any where in a straight line, bisecting the angle contained by the two lines given by position. In this case, the problem becomes a plane problem, and leads to an equation of the second degree only.

SINCE, then, the straight lines AB, CD, intersect one another at right angles, the plane case of the problem will happen, when the angle ACM is half a right angle; that is, when  $n$  is equal to an arch of  $45^\circ$ , or when the mean anomaly is a right angle. This case deserves to be particularly considered, on account of the simplicity of the solution it admits of.

WHEN  $n = 45^\circ$ , it is obvious, that  $\sin n = \cos n = \frac{1}{\sqrt{2}}$ : and the general equation becomes

$$e = \frac{1}{\sqrt{2}} \times \left( \frac{1}{\sin \nu} - \frac{1}{\cos \nu} \right) = \frac{1}{\sqrt{2}} \times \frac{\cos \nu - \sin \nu}{\sin \nu \cos \nu};$$

squaring both sides,

$$e^2 = \frac{1}{2} \times \frac{\cos^2 \nu + \sin^2 \nu - 2 \sin \nu \cos \nu}{\sin^2 \nu \cos^2 \nu} = \frac{1}{2} \times \frac{1 - 2 \sin \nu \cos \nu}{\sin^2 \nu \cos^2 \nu},$$

but  $\sin 2\nu = 2 \sin \nu \cos \nu$ : therefore

$$e^2 = 2 \times \frac{1 - \sin 2\nu}{\sin^2 2\nu}.$$

HAVING reduced this formula, we shall find

$$\sin^2 2\nu + \frac{2}{e^2} \sin 2\nu = \frac{2}{e^2}: \text{ whence } \sin 2\nu = \frac{\pm \sqrt{2e^2 + 1} - 1}{e^2}.$$

If we regard the problem, "*De inclinationibus*," independently of any application, and suppose that  $e$  may have all possible degrees of magnitude, both these values of  $\sin 2\nu$  may give solutions of the problem. One value, viz.  $\sin 2\nu = \frac{\sqrt{2e^2 + 1} - 1}{e^2}$ ,

being always less than 1, will give a solution, whatever be the magnitude of  $e$ : it will even give two solutions; because  $\sin 2\nu$ ,

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being

being determined in magnitude only, will correspond to two arches, the one less, and the other greater than  $90^\circ$ . Of these two arches, the former will give the position of the line FH, having the part GH, inscribed in the angle DCB, equal to CA; and the latter will give the position of the line FKL, having the part KL, inscribed in the angle ACL, equal to CA.

THE other value of  $\sin 2\nu$ , viz.  $\sin 2\nu = \frac{\sqrt{2e^2 + 1} - 1}{e^2}$ , will give no solution when  $e$  is not greater than 1; because, in that case,  $\frac{\sqrt{2e^2 + 1} + 1}{e^2}$  is greater than 1: But, for other values of  $e$ , when  $\frac{\sqrt{2e^2 + 1} + 1}{e^2}$  is less than 1, this value of  $\sin 2\nu$  will also determine the position of two straight lines, both lying in the angle ACD, that will satisfy the problem.

IN the particular application we have in view,  $e$  being never greater than 1, we must compute  $\sin 2\nu$  by the formula  $\sin 2\nu = \frac{\sqrt{2e^2 + 1} - 1}{e^2}$ : and all ambiguity will be taken away, by the consideration that  $2\nu$ , or the arch of eccentric anomaly, must be less than the arch of mean anomaly.

THE formula  $\sin 2\nu = \frac{\sqrt{2e^2 + 1} - 1}{e^2}$  is not very convenient in practice: we shall obtain another method of computing  $\sin 2\nu$ , greatly preferable in this respect, in the following manner.

RESUME the formula,

$$e^2 = 2 \times \frac{1 - \sin 2\nu}{\sin^2 2\nu}.$$

Suppose  $\sin 2\nu = \frac{2 \cos A}{1 + \cos A}$ : then, by substitution, we shall find

$$e^2 = \frac{1}{2} \times \frac{(1 - \cos A) \times (1 + \cos A)}{\cos^2 A} = \frac{\sin^2 A}{2 \cos^2 A} = \frac{1}{2} \times \tan^2 A;$$

therefore  $\tan A = e \times \sqrt{2} = e \times \sec 45^\circ$ .

Also,



Also, since  $\sin 2\nu = \frac{2 \cos A}{1 + \cos A}$ ; we have  $1 - \sin 2\nu =$

$$\frac{1 - \cos A}{1 + \cos A} = \frac{\sin^2 \frac{A}{2}}{\cos^2 \frac{A}{2}} = \tan^2 \frac{A}{2} : \text{now let } 2\nu = \mu = 90^\circ - 2\lambda;$$

then  $1 - \sin 2\nu = 1 - \cos 2\lambda = 2 \sin^2 \lambda$ : therefore  $\sin \lambda = \tan \frac{A}{2} \times \frac{1}{\sqrt{2}} = \tan \frac{A}{2} \times \sin 45^\circ$ .

INVERTING this analysis, we derive the following rule for computing the eccentric anomaly, when the mean anomaly is a right angle: Take  $\tan A = e \times \sec 45^\circ$ ; then  $\sin \lambda = \tan \frac{A}{2} \times \sin 45^\circ$ , and  $\mu = 90^\circ - 2\lambda$ . This rule would be rigorous and exact, if we could give to  $e$  the value it has in the

formula  $e = \varepsilon \times \frac{\sin \frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)}$ : but as this cannot be done, we

must be content with approximating to the arch sought as nearly as our purpose may require. We will, therefore, by means of the rule, compute a series of arches,  $p, p', p'', \&c.$  by successively

substituting for  $e$ , the values  $\varepsilon, \varepsilon \times \frac{\sin \frac{m - p}{2}}{\frac{1}{2}(m - p)}, \varepsilon \times \frac{\sin \frac{m - p'}{2}}{\frac{1}{2}(m - p')}, \&c.$

and the series  $p, p', p'', \&c.$  will converge very quickly to the exact value of the arch of eccentric anomaly, erring alternately in defect and in excess. For the arches here denoted by  $p, p', p'', \&c.$  are manifestly no other than the double of the arches formerly denoted by  $\pi, \pi', \pi'', \&c.$  respectively.

THE case of the general problem that we have here resolved, may be thus enunciated: "To draw a straight line from an  
" eccentric

“eccentric point in the diameter of a semicircle, that shall divide the semicircle into two equal parts.”

II. LET us now proceed to consider the resolution of the formula,

$$e = \frac{\sin n}{\sin v} - \frac{\cos n}{\cos v},$$

supposing  $n$  to be any angle whatever not greater than a right angle.

FROM the point  $F$ , draw the straight line  $FKL$ , so that the part  $KL$ , inscribed in the angle  $ACL$ , may be equal to  $CA$ ; and put  $q = \text{angle } FKA$ . Then, by proceeding in a manner similar to what is done in Art. 9. we shall easily derive this equation,

$$e = \frac{\cos n}{\cos q} - \frac{\sin n}{\sin q}.$$

FURTHER, let the straight line  $FQ$  be drawn to bisect the angle  $HFK$ : put  $\phi = \text{angle } FQA$ , and  $\psi = \text{angle } KFQ = \text{angle } QFH$ : then, since it has already been shewn, that  $v = \text{angle } FHA$ , it is obvious that  $v = \phi - \psi$  and  $q = \phi + \psi$ : whence we shall have these two equations, for determining the two angles  $\phi$  and  $\psi$ ,

$$e = \frac{\sin n}{\sin (\phi - \psi)} - \frac{\cos n}{\cos (\phi - \psi)};$$

$$e = \frac{\cos n}{\cos (\phi + \psi)} - \frac{\sin n}{\sin (\phi + \psi)}.$$

By taking away the denominators, and remarking, that  $\sin (\phi + \psi) \times \cos (\phi + \psi) = \frac{1}{2} \sin (2\phi + 2\psi)$  and  $\sin (\phi - \psi) \times \cos (\phi - \psi) = \frac{1}{2} \sin (2\phi - 2\psi)$ , there will result

$$\frac{e}{2} \sin (2\phi - 2\psi) = \sin n \times \cos (\phi - \psi) - \cos n \times \sin (\phi - \psi), \text{ and}$$

$$\frac{e}{2} \sin (2\phi + 2\psi) = \cos n \times \sin (\phi + \psi) - \sin n \times \cos (\phi + \psi).$$

TAKE the sum and difference of these two equations, and, for the sums and differences of the sines and co-sines, having

the same co-efficient, substitute the products that are equal to them; we shall have,

$$e \sin 2\phi \cos 2\psi = 2 \sin n \sin \phi \sin \psi + 2 \cos n \cos \phi \sin \psi, \text{ and}$$

$$e \cos 2\phi \sin 2\psi = -2 \sin n \cos \phi \cos \psi + 2 \cos n \sin \phi \cos \psi.$$

Now,  $\sin n \sin \phi + \cos n \cos \phi = \cos(\phi - n)$ , and  
 $-\sin n \cos \phi + \cos n \sin \phi = \sin(\phi - n)$ : therefore,

$$\left. \begin{aligned} e \sin 2\phi \cos 2\psi &= 2 \cos(\phi - n) \sin \psi \\ e \cos 2\phi \sin 2\psi &= 2 \sin(\phi - n) \cos \psi \end{aligned} \right\} (A)$$

If in the second of the equations (A), we write  $2 \sin \psi \cos \psi$  for  $\sin 2\psi$ , and divide by  $\cos \psi$ , we shall obtain  $e \cos 2\phi \sin \psi = \sin(\phi - n)$ :

whence  $\sin \psi = \frac{1}{e} \times \frac{\sin(\phi - n)}{\cos 2\phi}$ . But  $\cos 2\psi = 1 - 2 \sin^2 \psi =$

$$1 - \frac{2}{e^2} \times \frac{\sin^2(\phi - n)}{\cos^2 2\phi}.$$

Substitute these values of  $\sin \psi$  and  $\cos 2\psi$  in the first of the equations (A), and, after having reduced, there will result,

$$e^2 \sin 2\phi \cos^2 2\phi - 2 \sin^2(\phi - n) \sin 2\phi = 2 \sin(\phi - n) \cos(\phi - n) \cos 2\phi.$$

Now,  $2 \sin^2(\phi - n) = 1 - \cos(2\phi - 2n)$ , and  $2 \sin(\phi - n) \times \cos(\phi - n) = \sin(2\phi - 2n)$ : therefore, by properly ordering the terms,

$$\sin 2\phi - e^2 \sin 2\phi \cos^2 2\phi = \sin 2\phi \cos(2\phi - 2n) - \cos 2\phi \sin(2\phi - 2n).$$

But  $\sin 2\phi \cos(2\phi - 2n) - \cos 2\phi \sin(2\phi - 2n) = \sin 2n$ : therefore, if we put  $x = \sin 2\phi$ , since  $\cos^2 2\phi = 1 - x^2$ , our equation will finally become

$$x^3 + \left( \frac{1}{e^2} - 1 \right) x = \frac{\sin 2n}{e^2},$$

which equation will serve to determine  $x = \sin 2\phi$ .

We have still to find the angle  $\psi$ : for this purpose I resume the equations (A), and multiplying cross-wise, and dividing by  $2e$ , there results,

$$\sin 2\phi \sin(\phi - n) \cos 2\psi \cos \psi = \cos 2\phi \cos(\phi - n) \sin 2\psi \sin \psi.$$

For  $\sin 2\psi$  write  $2 \sin \psi \cos \psi$ : and for  $2 \sin^2 \psi$  write  $1 - \cos 2\psi$ ; then, having properly disposed the terms, we shall find

$$(\sin 2\phi \sin(\phi - n) + \cos 2\phi \cos(\phi - n)) \cos 2\psi = \cos 2\phi \cos(\phi - n).$$

Now.

Now,  $\sin 2\phi \sin (\phi - n) + \cos 2\phi \cos (\phi - n) = \cos (\phi + n)$ :  
therefore

$$\cos (\phi + n) \times \cos 2\psi = \cos 2\phi \times \cos (\phi - n),$$

whence, as  $\phi$  is now known,  $\cos 2\psi$  will be found by this proportion,

$$\cos (\phi + n) : \cos (\phi - n) :: \cos 2\phi : \cos 2\psi.$$

HAVING thus found both the angles  $\phi$  and  $\psi$ , their difference will give the angle  $\nu$  which is sought.

IN order to render the method of computation, derived from the preceding analysis, as simple and as commodious for practice as the nature of the subject will permit, we shall change the letters  $\phi$  and  $\psi$  to denote the double of what they have hitherto done; and we shall also put  $m$  for its equal  $2n$ : This being observed, we have the following rule:

1. Let there be formed this cubic equation

$$x^3 + \left(\frac{1}{e^2} - 1\right)x = \frac{\sin m}{e^2},$$

from which  $x$  is to be found: then  $x = \sin \phi$ .

2. STATE this proportion,

$$\cos \frac{\phi + m}{2} : \cos \frac{\phi - m}{2} :: \cos \phi : \cos \psi,$$

by which the angle  $\psi$  will be found.

THEN  $\phi - \psi = 2\nu =$  arch of eccentric anomaly.

THIS rule would be rigorous and exact if we could give to  $e$

the value it has in the formula  $e = \epsilon \times \frac{\sin \frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)}$ : and if, by

means of the rule, we compute a series of arches,  $p, p', p'', \&c.$

by successively substituting for  $e$ , the values  $\epsilon, \epsilon \times \frac{\sin \frac{m - p}{2}}{\frac{1}{2}(m - p)},$

$\epsilon \times$

$\epsilon \times \frac{\sin \frac{m-p'}{2}}{\frac{1}{2}(m-p')}$ , &c.; the series  $p, p', p'',$  &c. will converge very

quickly to the exact value of the arch of eccentric anomaly, erring alternately in defect and in excess. For the arches that we here denote by  $p, p', p'',$  &c. are manifestly no other than the doubles of the arches formerly denoted by  $\pi, \pi', \pi'',$  &c. respectively.

12. It is to be remarked, that, since  $\epsilon$  is never greater than 1, the cubic equation in the rule above, is of the form which admits only one real root, so that it may either be resolved by CARDAN'S rule, or by the ordinary methods of approximation; its root is always positive.

It is to be remarked too, that in the same cubic equation,  $x = 1$ , or  $\sin \phi = 1$  when  $\sin m = 1$ ; and, consequently,  $\phi = 90^\circ$  when  $m = 90^\circ$ . Hence, it is easy to infer, that the arch  $\phi$  is less, or greater than a quadrant, or equal to it, according as the arch  $m$  is less, or greater than a quadrant, or equal to it. This remark is necessary to determine  $\phi$ , when its sine  $x$  is given, on account of the ambiguity of the sines.

When  $m = 90$ , or  $\sin m = 1$ , we shall manifestly have the case of the problem that we before (Art. 10.) considered separately. But though we have here  $\sin \phi = 1$ , and  $\phi = 90^\circ$ , we shall in vain look for a solution of this case from the general rule, (Art. 11.): because the first and third terms in the proportion for computing  $\cos \psi$  become evanescent. We may, however, deduce the rule of calculation of Art. 10. from the general investigation of Art. 11. in the following manner:

RESUME the first of the equations (A), writing  $\phi, \psi$  and  $m$ , for  $2\phi, 2\psi$  and  $2n$ , according to the change made in the notation: viz.

$$\epsilon \sin \phi \times \cos \psi = 2 \cos \frac{\phi-m}{2} \times \sin \frac{\psi}{2}.$$

Suppose  $\text{cof } \psi = \frac{2 \text{ cof } A}{1 + \text{cof } A}$ : then, since  $1 - \text{cof } \psi = 2 \sin^2 \frac{\psi}{2}$

we have  $2 \sin^2 \frac{\psi}{2} = \frac{1 - \text{cof } A}{1 + \text{cof } A} = \frac{\sin^2 \frac{A}{2}}{\text{cof}^2 \frac{A}{2}} = \tan^2 \frac{A}{2}$ , therefore

$\sin \frac{\psi}{2} = \tan \frac{A}{2} \times \frac{1}{\sqrt{2}} = \tan \frac{A}{2} \times \sin 45^\circ$ : We shall also have

$2 \sin^2 \frac{\psi}{2} = \frac{1 - \text{cof } A}{1 + \text{cof } A} = \frac{(1 - \text{cof } A)^2}{1 - \text{cof}^2 A} = \frac{(1 - \text{cof } A)^2}{\sin^2 A}$ ; whence

$\sin \frac{\psi}{2} = \frac{1 - \text{cof } A}{\sin A} \times \frac{1}{\sqrt{2}}$ : Substitute now, for  $\text{cof } \psi$ , its value

$\frac{2 \text{ cof } A}{1 + \text{cof } A}$ ; and for  $\sin \frac{\psi}{2}$ , its value  $\frac{1 - \text{cof } A}{\sin A} \times \frac{1}{\sqrt{2}}$ ; and we shall easily obtain,

$$e \sqrt{2} \times \sin \phi \times \text{cof } A = \text{cof} \frac{\phi - m}{2} \times \sin A,$$

whence  $\frac{\sin A}{\text{cof } A} = \tan A = e \times \frac{\sin \phi}{\text{cof} \frac{\phi - m}{2}} \times \sqrt{2}$ .

Hence we have another general rule for computing  $\psi$  when  $\phi$  is given, which is this: Take  $\tan A = e \times \frac{\sin \phi}{\text{cof} \frac{\phi - m}{2}} \times \sec 45^\circ$ ;

then  $\sin \frac{\psi}{2} = \tan \frac{A}{2} \times 45^\circ$ .

TAKING  $m = 90^\circ$ , and  $\phi = 90^\circ$ , this rule coincides with that already given in Art. 10.: And it may be of use, not only when  $m$  is exactly a right angle, but also when it is very nearly so.

THE detail with which we have discussed the rules and formulas of computation that have been deduced from the analysis, leaves no difficulty in applying them to practice. These rules are

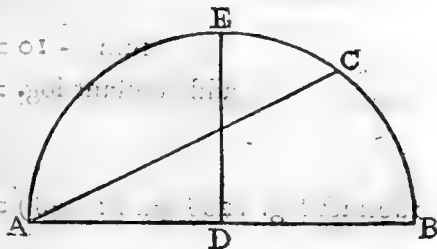
are sufficient, in all cases whatever, for computing the eccentric anomaly, when the mean anomaly is given. They embrace the problem in its fullest extent, and, in point of universality, nothing more is to be wished for.

THUS, then, we have a general and direct method of determining the motion of a body describing an elliptic orbit, whether the eccentricity of the orbit be small or great. The method is so extensive, as even to comprehend the case, when the elliptic orbit, having become indefinitely flattened, the motion of the body is no longer in a curve, but in a straight line, tending to the centre of forces. (*Vide Prin. Math. lib. 1. sect. 7. prop. 32. et 36.*)

13. IN order to illustrate the method of computation required in the rules that have been investigated, I shall now subjoin two examples. I have selected, for this purpose, two problems relating to the circle, taken from a work of M. EULER, (*Int. in Analyf. Inf. lib. xi. cap. 22. prob. 4. et 5.*) where they are resolved by the method of *trial and error*.

EXAMPLE 1. Prob. To draw a chord, AC, from the extremity of the diameter of a semicircle, that shall divide the semicircle into two equal parts.

TAKE D, the centre of the circle, and draw DE perpendicular to AB: It is manifest, that the sector BDE will be equal to the sector BAC; and that BE, being the mean anomaly, BC will be the anomaly of the eccentric. We have here, then,  $m = 90^\circ$



and  $\varepsilon = 1$ : and we must compute by the rule in Art. 10.

1. To compute  $p$ , the first term in the series, approximating to BC: Since  $e = \varepsilon = 1$ , we have

$$\tan A = \sec 45^\circ = 10.1505150,$$

therefore  $A = 54^\circ 44'$ .

Then  $\sin \lambda = \tan \frac{A}{2} \times \sin 45^\circ$ ; and  $p = 90^\circ - 2\lambda$ .

$$\text{Now } \log. \tan \frac{A}{2} = 9.7140051$$

$$\log. \sin 45^\circ = 9.8494850$$

$$\log. \sin \lambda = 9.5634901 = \sin 21^\circ 28'$$

Therefore  $\lambda = 21^\circ 28''$ , and  $p = 90^\circ - 2\lambda = 47^\circ 4'$ ; this value of  $p$  is less than BC.

$$2. \text{ To compute the second term } p', \text{ we have } e = \varepsilon \times \frac{\sin \frac{m-p}{2}}{\frac{1}{2}(m-p)}$$

$$= \frac{\sin 21^\circ 28'}{\arcsin 21^\circ 28'} \text{ and } \tan A = e \times \sec 45^\circ = \frac{\sin 21^\circ 28'}{\arcsin 21^\circ 28'} \times \sec 45^\circ.$$

$$\log. \sec 45^\circ = 10.1505150,$$

$$\log. \sin 21^\circ 28' = 9.5634335$$

$$\text{sum} - 10 = 9.7139485$$

$$\text{add constant log.} = 3.5362739$$

$$13.2502224$$

$$\text{subtract log. } 1288' (= 21^\circ 28') = 3.1099159$$

$$\log. \tan A = 10.1403065$$

therefore  $A = 54^\circ 5' 54''$ .

Then,



Then,  $\sin \lambda = \tan \frac{A}{2} \times \sin 45^\circ$ , and  $p' = 90^\circ - 2\lambda$ ,

$$\log. \tan \frac{A}{2} = 9.7080866$$

$$\log. \sin 45^\circ = 9.8494850$$

$$\log. \sin \lambda = 9.5575716 = \log. \sin 21^\circ 9' 53'',$$

therefore,  $2\lambda = 42^\circ 19' 46''$ , and  $p' = 47^\circ 40' 14''$ ; this value of  $p'$  is greater than BC.

3. FOR the third term  $p''$ , we have  $e = \epsilon \times \frac{\sin \frac{m-p'}{2}}{\frac{1}{2}(m-p')} =$

$$\frac{\sin 21^\circ 10'}{\text{arc } 21^\circ 10'}; \text{ and } \tan A = e \times \sec 45^\circ = \frac{\sin 21^\circ 10'}{\text{arc } 21^\circ 10'} \times \sec 45^\circ.$$

$$\log. \secant 45^\circ = 10.1505150$$

$$\log. \sin 21^\circ 10' = 9.5576060$$

$$\text{sum} - 10 = 9.7081210$$

$$\text{add const. log.} = 3.5362739$$

$$13.2443949$$

$$\text{subtract log. } 1270' = 3.1038037$$

$$\log. \tan A = 10.1405912$$

therefore  $A = 54^\circ 6' 58''$ .

Now  $\sin \lambda = \tan \frac{A}{2} \times \sin 45^\circ$ , and  $p'' = 90^\circ - 2\lambda$ ;

$$\log. \tan \frac{A}{2} = 9.7082530$$

$$\log. \sin 45^\circ = 9.8494850$$

$$\log. \sin \lambda = 9.5577380 = \log. \sin 21^\circ 10' 24''.$$

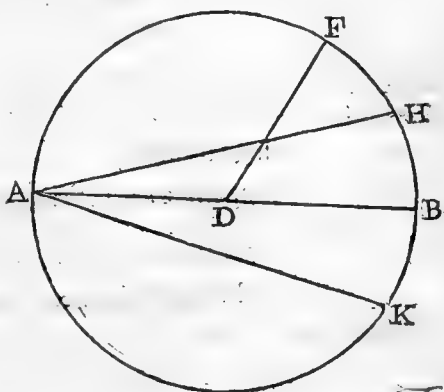
Therefore  $2\lambda = 42^\circ 20' 48''$ , and  $p'' = 47^\circ 39' 12''$ ; this value of  $p''$  is less than BC.

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THE more accurate value of BC, according to the computation of M. EULER, is  $47^{\circ} 39' 12'' 46'''$ : so that the last approximation is already almost exact. This example is well fitted to illustrate the convergency of the series,  $p, p', p'', \&c.$  even in the most unfavourable circumstances.

EXAMPLE 2. Prob. From a given point A, in the circumference of a circle, to draw two chords, AH and AK, that shall divide the circle into three equal parts.

DRAW the diameter AB, and, from the centre D, draw the radius DF, making the angle  $BDF = 60^{\circ}$ . Because the segment AH is one-third part of the whole circle, it will be two-third parts of the semi-circle: therefore the sector HAB will be one-third part of the semi-circle, and will be equal to the sector BDF.



Wherefore it is evident, that BF, being the mean anomaly, BH will be the anomaly of the eccentric: so that in this case we have  $m = 60$ , and  $e = 1$ .

1. To compute the first term  $p$ : we have  $e = e = 1$ , and the cubic equation becomes simply,  $x^3 = \sin m$ ; whence  $x = \sin \phi = \sqrt[3]{\sin m} = \sqrt[3]{\sin 60^{\circ}}$ : therefore

$$\log. \sin \phi = 9.9791769,$$

$$\text{and } \phi = 72^{\circ} 24'.$$

$$\text{Now } \cos \frac{\phi + m}{2} : \cos \frac{\phi - m}{2} :: \cos \phi : \cos \psi,$$

eof

$$\log. \cos \phi = 9.4805385$$

$$\log. \cos \frac{\phi - m}{2} = 9.9974523$$

$$\hline 19.4779908$$

$$\log. \cos \frac{\phi + m}{2} = 9.6058923$$

$$\log. \cos \psi = 9.8720985$$

therefore  $\psi = 41^\circ 51'$ ,

consequently  $p = \phi - \psi = 30^\circ 33'$ , which is less than BH.

2. For the next term  $p'$ , we have  $e = e \times \frac{\sin \frac{m - p}{2}}{\frac{1}{2}(m - p)} =$

$$\frac{\sin 14^\circ 43' 30''}{\text{arc } 14^\circ 43' 30''}; \text{ and } x^3 + \left(\frac{1}{e^2} - 1\right)x = \frac{\sin m}{e^2};$$

$$\log. \sin 14^\circ 43' 30'' = 9.4051412$$

$$\text{add const. log.} = 3.5362739$$

$$\hline \text{sum} - 10 = 2.9414151$$

$$\text{subtract log. } 883'.5 = 2.9462066$$

$$\hline \log. e = 1.9952085$$

$$\log. \frac{1}{e} = 0.0047915$$

$$\hline 2$$

$$\log. \frac{1}{e^2} = 0.0095830, \text{ and } \frac{1}{e^2} = 1.022410,$$

$$\text{also } \frac{1}{e^2} - 1 = .022410 = a. \text{ Now, } \log. \sin m = 9.9375306,$$

$$\text{therefore } \log. \frac{\sin m}{e^2} = 9.9471136, \text{ and } \frac{\sin m}{e^2} = .885347 = b.$$

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The cubic equation therefore becomes  $x^3 + ax = b$ : and it is manifest that  $x$  is nearly  $= \sin 72^\circ 24'$ ; and, having corrected this value, by the ordinary method, I find,

$$x = \sin \phi = .9524420$$

therefore  $\phi = 72^\circ 15' 31''$ .

$$\text{But, } \operatorname{cof} \frac{\phi + m}{2} : \operatorname{cof} \frac{\phi - m}{2} :: \operatorname{cof} \phi : \operatorname{cof} \psi,$$

$$\log. \operatorname{cof} \phi = 9.4839026$$

$$\log. \operatorname{cof} \frac{\phi - m}{2} = 9.9975102$$

$$19.4814128$$

$$\log. \operatorname{cof} \frac{\phi + m}{2} = 9.6071052$$

$$\log. \operatorname{cof} \psi = 9.8743076.$$

Therefore  $\psi = 41^\circ 31' 20''$ , and

consequently  $p' = \phi - \psi = 30^\circ 44' 11''$ , which is greater than BH.

M. EULER finds the arch  $AH = 129^\circ 16' 27''$ ; therefore,  $BH = 30^\circ 43' 33''$ : so that the second approximation  $p'$ , differs little more than half a minute of a degree from the truth.

14. THE only cases of KEPLER's problem that are interesting to the astronomical observer, are the two extreme cases, when the eccentricity is very small, and when it is very great, approaching to unity. The former of these two cases is that of the planets, all of which describe orbits very little eccentric, and nearly circular; the latter is that of the comets, which, on the contrary, move in very eccentric orbits. The principal object of this paper is accomplished in what has already been done; but it will be no improper sequel, to apply the general method to the two cases just mentioned.

THE supposition that the eccentricity is small, contributes greatly to remove the chief difficulties that occur in the solution  
of

of KEPLER'S problem. Indeed, it is only this particular case of the general problem, that we can consider to have been resolved, hitherto, in a satisfactory manner. We already possess many excellent solutions of this case, some of them deduced from the most elementary principles, and others obtained by the aid of the higher calculus. To these another may be added, derived as a corollary from the general solution contained in this paper, and which will be found not unworthy of the notice of astronomers.

BECAUSE the two arches denoted by  $\mu$  and  $p$  are the double of the arches  $\nu$  and  $\pi$ , the arch  $\mu - p$ , which is the error of  $p$ , considered as an approximation to the eccentric anomaly, will be double of the arch  $\nu - \pi$ : therefore, it is obvious, (Art. 8.) that the arch  $\mu - p$  will never be greater than

$$2 \left( \varepsilon - \tan \varepsilon \sqrt{1 - \frac{\varepsilon^2}{4}} \right),$$

$$\text{taking } \tan \varepsilon = \sqrt{\frac{1 - \sqrt{1 - \frac{\varepsilon^2}{4}}}{1 + \sqrt{1 - \frac{\varepsilon^2}{4}}}}.$$

THE slightest attention to the nature of this expression, is sufficient to evince, that it decreases very rapidly as  $\varepsilon$  decreases. If we evolve it into a series, proceeding according to the powers of  $\varepsilon$ , that series will contain only the third and higher odd powers. Therefore, when  $\varepsilon$  is small, as it is in the case of the planets, the amount of the above formula will be inconsiderable. It may even be so inconsiderable, that the error of  $p$  will be of no account in practice, and the first approximation will give the eccentric anomaly with the requisite exactness.

By means of this formula, I have computed the limit of the error of  $p$ , for all the planetary orbits, and have arranged the results in the following table:

	Value of $\epsilon$	Limit of the error of $p$
Mercury,	.205513	1' 46".0
Venus,	.006885	0.0
Earth,	.016814	0.0
Mars,	.093088	9.8
Jupiter,	.048077	1.3
Saturn,	.056223	2.1
Georgium Sidus,	.046683	1.2

The inspection of this table shews, that the error of the first approximation, obtained by the supposition of  $e = \epsilon$ , in all the planetary orbits, is a very small quantity, and such as may be neglected on most occasions.

It is to be recollected, that the rule we have investigated for computing the eccentric anomaly, would give a rigorous result, provided the exact value of  $e$  were known. But, as that value cannot be deduced directly from the data, the repetition of the calculation is necessary to correct the first assumed value of  $e$ , and to make it approach nearer and nearer to the true value. The method of proceeding that is directed above, viz. to assume at first  $e = \epsilon$ , and from thence to deduce a series of approximations to the arch sought, is perhaps the only one that will apply universally, and in all circumstances of the problem. But it is to be observed, that the reasoning in Art. 4, 5, and 6. will remain the same, provided only that the first assumed value of  $e$  be greater than its true value: and, if the first assumed value of  $e$  be less than its true value, the reasoning will not thereby be essentially altered; all the change that will take place, is, that the error of the approximations will now be alternately in excess and in defect. Therefore, in applying the general method, if we can take hold of any circumstances, peculiar to the parti-

cular:

cular case, from which we can directly deduce a nearer value of  $e$  than the supposition of  $e = \varepsilon$ , we will avail ourselves of such circumstances, and will thereby obtain a series of approximations, converging faster to the eccentric anomaly.

RESUME the general equation of the arches of eccentric and mean anomalies. (Art. 2.) viz.

$$m - \mu = \varepsilon \sin \mu :$$

it is evident, that the less  $\varepsilon$  is, the less will be the difference of the two arches  $m$  and  $\mu$ ; and that, when  $\varepsilon$  is small, the quantity  $\varepsilon \sin \mu$  will be nearly equal to the quantity  $\varepsilon \sin m$ . Therefore, if we take an arch  $\tau$ , such that  $2\tau = \varepsilon \sin m$ , it is ob-

vious that we shall have  $\frac{\sin \tau}{\tau} = \frac{\sin \frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)}$  nearly, and, conse-

quently,  $e = \varepsilon \times \frac{\sin \tau}{\tau}$  nearly. To speak more correctly, the error of the assumption  $e = \varepsilon$ , will be of the same order with the third power of the eccentricity; but the error of the assumption,  $e = \varepsilon \frac{\sin \tau}{\tau}$ , will be of the same order with the fourth power of the eccentricity\*.

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\* SINCE  $m - \mu = \varepsilon \sin \mu$ , we have, in series,

$$\frac{\sin m - \mu}{2} = \frac{\varepsilon \sin m}{2} - \frac{1}{6} \varepsilon^3 \frac{\sin^3 \mu}{2^3} + \frac{3}{40} \varepsilon^5 \frac{\sin^5 \mu}{2^5} - \&c.$$

$$\text{Therefore } \frac{\sin \frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)} = 1 - \frac{\varepsilon^2}{24} \sin^2 \mu + \frac{3\varepsilon^4}{640} \sin^4 \mu - \&c.$$

$$\text{and } e = \varepsilon \times \frac{\sin \frac{m - \mu}{2}}{\frac{1}{2}(m - \mu)} = \varepsilon - \frac{\varepsilon^3}{24} \sin^2 \mu + \frac{3\varepsilon^5}{640} \sin^4 \mu - \&c.$$

so that the error of the supposition  $e = \varepsilon$ , is manifestly of the order  $\varepsilon^3$ .

AGAIN, from the equation  $m - \mu = \varepsilon \sin \mu$ , we easily derive  $\sin \mu = \sin m - \frac{\varepsilon}{2} \sin 2m$ , neglecting the terms above the first order: and, substituting

this

It has been shewn above, that the error of the first approximation, derived from the assumption  $e = \varepsilon$ , is almost of no account, as to any real practical purpose, in the orbits of all the planets, excepting Mercury: and much more will this conclusion be true of the more exact assumption  $e = \varepsilon \frac{\sin \tau}{\tau}$ .

LET us now consider the cubic equation which the rule requires to be resolved: the equation is

$$x^3 + \left( \frac{1}{e^2} - 1 \right) x = \frac{\sin m}{e^2},$$

and, in the case we are now occupied with,  $e$  is small, being nearly equal to the eccentricity. Multiply all the terms of the equation by  $e^2$ ; write  $\sin \phi$  for  $x$ , and  $\cos^2 \phi$  for  $1 - x^2$ ; and we shall easily obtain,

$$\sin \phi = \sin m + e^2 \sin \phi \cos^2 \phi.$$

In this formula it is clear, that the term  $e^2 \sin \phi \cos^2 \phi$  is inconsiderable in comparison of the other two: therefore  $\sin \phi = \sin m$  nearly; and, consequently, the two arches  $\phi$  and  $m$  will differ but little from one another. From this consideration, we readily derive a series of approximations,  $\phi'$ ,  $\phi''$ ,  $\phi'''$ , &c. converging very fast to the exact value of  $\phi$ ; viz.

$$\sin \phi' = \sin m + e^2 \sin m \cos^2 m$$

$$\sin \phi'' = \sin m + e^2 \sin \phi' \cos^2 \phi'$$

$$\sin \phi''' = \sin m + e^2 \sin \phi'' \cos^2 \phi'';$$

and so on. The error of the first approximation  $\phi'$ , is of the order

this value of  $\sin \mu$  in the series for  $e$ , we have, (neglecting the terms above the order  $\varepsilon^4$ ),

$$e = \varepsilon - \frac{\varepsilon^3}{24} \sin^2 m + \frac{\varepsilon^4}{24} \sin m \times \sin 2m;$$

and this value of  $e$  is exact, as far as the order  $\varepsilon^4$  inclusively.

BUT the assumption  $e = \varepsilon \times \frac{\sin \tau}{\tau}$ , where  $\tau = \frac{\varepsilon \sin m}{2}$ , being thrown into a series, we get,

$$e = \varepsilon - \frac{\varepsilon^3}{24} \sin^2 m + \frac{\varepsilon^5}{640} \sin^4 m - \&c.$$

and therefore the error of this assumption is of the order  $\varepsilon^4$ .



order  $\epsilon^4$  or  $\epsilon^1$ ; that of the second approximation is of the order  $\epsilon^5$ ; and in none of the planetary orbits will it be necessary to push the approximations further than the second term of the series.

THE method of finding the arch  $\phi$ , that has just been explained, is very commodious in practice; because the value of  $\epsilon^2 \sin \phi \cos^2 \phi$  is easily computed by the common tables, when a known arch is substituted for  $\phi$ . But we may, with advantage, apply the method of infinite series to the resolution of the equation. If we put  $\beta^2 = 1 - \epsilon^2$ , and  $z = \frac{\sin m}{\beta^2} \times \frac{\epsilon}{\beta}$ , we have

$$\sin \phi = \frac{\sin m}{\beta^2} \times (1 - z^2 + 3z^4 - 12z^6 + 55z^8 - 8\&c.).$$

THE rule requires still another operation, viz. to find the arch  $\psi$ . For this purpose we have the proportion

$$\cos \frac{\phi + m}{2} : \cos \frac{\phi - m}{2} :: \cos \phi : \cos \psi.$$

But, in the case we are now considering, the arch  $\psi$  is always small: and, on this account, the proportion above is of little use in practice, when any degree of accuracy is required. The reason is, that the common tables are not computed to a sufficient number of figures for determining small angles from their cosines. We will, therefore, prefer the other method of computing  $\psi$ , given in Art. 12. which is not liable to the same inconvenience.

THE observations we have now made, lead us to the following rule, for computing the anomaly of the eccentric in the orbits of the planets:

1. COMPUTE the arch  $\tau$  from the formula  $2\tau = \epsilon \times \sin m$ .
2. COMPUTE  $e = \epsilon \times \frac{\sin \tau}{\tau}$ ; and determine the arch  $\phi$  from the equation  $\sin \phi = \sin m + \epsilon^2 \sin \phi \cos^2 \phi$ .
3. COMPUTE  $\tan A = e \times \frac{\sin \phi}{\cos \frac{\phi - m}{2}} \times \sec 45^\circ$ ; and  $\sin \frac{\psi}{2}$

$$= \tan \frac{A}{2} \times \sin 45^\circ.$$

THEN  $\mu = \phi - \psi$ .

It

It is to be kept in mind, that the arch found by this rule is only the first term of a series of approximations, converging very fast to the eccentric anomaly: and that, by a repetition of the calculation, a result may be obtained, that will satisfy the most scrupulous accuracy. But the rule may be considered as exact, as to any real practice, for the orbits of all the planets, excepting Mercury: and, even in the orbit of Mercury, the error will never exceed a few seconds. Let it be observed, further, that the error of the rule is chiefly in the arch  $\psi$ : for the error of  $\psi$  is of the same order with the error of  $e$ ; whereas the error of  $\phi$  is of the same order with the error of  $e^2$ .

EXAMPLE. Let it be required to find the eccentric anomaly, corresponding to the mean anomaly  $64^\circ 37' 8''.5$  in the orbit of Mars, supposing the eccentricity to be  $= .093088$ .

WE have here  $m = 64^\circ 37' 8''.5$ , and  $\varepsilon = .093088$ .

1. To compute  $\tau$  from the formula  $2\tau = \varepsilon \sin m$ ;

$$\log. \varepsilon = \overline{2.9688937}$$

$$\log. \sin m = 9.9559089$$

$$\text{const. log.} = \underline{3.5362739}$$

$$\log. 2\tau \text{ in minutes} = 2.4610765; 2\tau = 289'; \text{ and } \tau = 2^\circ 24'.$$

2. THEN  $e = \varepsilon \frac{\sin \tau}{\tau}$ ; and  $\sin \phi' = \sin m + e^2 \sin m \cos^2 m$ .

$$\sin \tau = 8.6219616$$

$$\log. \varepsilon = \overline{2.9688937}$$

$$\text{const. log.} = \underline{3.5362739}$$

$$\text{sum} - 10 = \underline{1.1271292}$$

$$\text{subtract log. } 144' = \underline{2.1583625}$$

$$\log. e = \overline{2.9687667}$$

$$\log. e^2 = \overline{3.9375334}$$

$$\log. \sin m = 9.9559089$$

$$2 \log. \cos m = \underline{19.2642510}$$

$$\log. e^2 \sin m \cos^2 m = \overline{3.1576933}, \text{ and } e^2 \sin m \cos^2 m = .0014378.$$

To

To this adding nat. fin  $m = .9034776$ , we have nat. fin  $\phi' = .9049154$ , and  $\phi' = 64^\circ 49'$ . To have a more correct value of  $\phi$ , repeat the calculation,

$$\text{Log. } e^2 = \overline{3.9375334}$$

$$\text{log. fin } \phi' = 9.9566250$$

$$2 \times \text{log. cof } \phi' = \underline{19.2578320}$$

$\text{log. } e^2 \text{ fin } \phi' \text{ cof}^2 \phi' = \overline{3.1519904}$ , whence nat. fin  $\phi = .9048966$ , therefore  $\phi = 64^\circ 48' 33''$ .

$$3. \text{ To find } \psi, \text{ we have } \tan A = e \times \frac{\text{fin } \phi}{\text{cof } \frac{\phi - m}{2}} \times \sec 45^\circ;$$

$$\text{log. } e = \overline{2.9687667}$$

$$\text{log. fin } \phi = 9.9565982$$

$$\text{log. sec } 45^\circ = \underline{10.1505150}$$

$$\text{Sum} = 19.0758799$$

$$\text{log. cof } \frac{\phi - m}{2} = \underline{9.9999994}$$

$$\text{log. tan } A = 9.0758805 = \text{log. tan } 6^\circ 47' 29''$$

But  $\text{fin } \frac{\psi}{2} = \tan \frac{A}{2} \times \text{fin } 45^\circ$ , therefore

$$\text{log. tan } \frac{A}{2} = 8.7733146$$

$$\text{log. fin } 45^\circ = \underline{9.8494850}$$

$$\text{log. fin } \frac{\psi}{2} = 8.6227996 = \text{log. fin } 2^\circ 24' 16''.5. \text{ Therefore}$$

$\frac{\psi}{2} = 2^\circ 24' 16''.5$ , and  $\psi = 4^\circ 48' 33''$ , so that  $\mu = \phi - \psi = 60^\circ$ , the eccentric anomaly required.

15. It remains now to consider the case of the problem, applicable to comets, where the eccentricity is very great, or nearly equal to unity. Here the general solution may be used, and, even without any new simplification, arising from the peculiarities of the case, will bring out an accurate result with very little trouble:

trouble. I shall give an example in the orbit of the famous comet of 1682.

THE comet of 1682, which re-appeared in 1759, according to the prediction of Dr HALLEY, is the only one of which the period is known with any tolerable degree of certainty. M. de la LANDE has fixed the period of this comet at 28070 days: computing from this the mean distance from the sun, by the law of KEPLER, that the cubes of the mean distances are as the squares of the periodic times, we shall find, that half the greater axis of the ellipse described by the comet, is 18.07575, the mean distance of the earth from the sun being unity. According to the determination of the same astronomer, the perihelion distance, estimated in parts of the same unit, is 0.5835; consequently, the distance of the focus of the ellipse from the centre, is 17.49225. Therefore, in the orbit of this comet, the eccentricity, or the quantity  $\epsilon$ , is equal to  $\frac{17.49225}{18.07575} = 0.96772$  nearly: and we can now assign the true place of the comet in the orbit, as well as its true distance from the sun, at any given distance of time, from the passage over the perihelion or aphelion.

EXAMPLE. Let it be required to find the anomaly of the eccentric of the comet 1759, (from which the true place and true distance from the sun are derived by easy and known rules), 16 days, 4 hours, 44' before or after the passage over the perihelion.

The mean anomaly, corresponding to the given time, is  $0^{\circ} 12' 27''.83$ , reckoned from the perihelion; but, as our method requires the mean anomaly to be reckoned from the aphelion, we have  $m = 179^{\circ} 47' 32''.17$ .

I. To compute the first approximation to the eccentric anomaly sought, we have  $e = \epsilon = 0.96772$ ; hence  $\frac{1}{e^2} = 1.0678$ ;  $\frac{1}{e^2} - 1 = 0.0678 = a$ ;  $\frac{\sin m}{e^2} = 0.0038715 = b$ .

Then,

Then, to find a value of  $x$ , from the equation,  $x^3 + ax = b$ : since  $b$  is small in comparison of  $a$ , it is manifest that  $x$  must be a very small fraction; and, consequently,  $x^3$  inconsiderable in respect of  $ax$ : therefore  $x = \frac{b}{a} = .05$  nearly: and, having corrected this value by the common method, we shall find,

$$x = \sin \phi = .0547 = \sin 3^\circ 8', \text{ and so } \phi = 176^\circ 52'.$$

As the first approximation which we are now computing, cannot be exact, even to the nearest minute, it would be fruitless to push the calculation to a great degree of accuracy. For the same reason, I here use the proportion in the general rule, because it requires but one operation for finding  $\psi$ , viz.  $\cos \frac{\phi + m}{2}$ :

$$\cos \frac{\phi - m}{2} :: \cos \phi : \cos \psi.$$

HENCE  $\psi$  is found  $= 3^\circ 1'$ , and therefore  $p = \phi - \psi = 173^\circ 51'$ ; this is the first approximation to the eccentric anomaly, reckoned from the aphelion, and is too small.

2dly, To compute the second approximation, we have,

$$e = e \times \frac{\sin \frac{m - p}{2}}{\frac{1}{2}(m - p)} = e \times \frac{\sin 2^\circ 58'}{\text{arc } 2^\circ 58'}; \text{ whence } \frac{1}{e^2} = 1.06878;$$

$$\frac{1}{e^2} - 1 = .06878 = a; \quad \frac{\sin m}{e^2} = .0038749 = b. \quad \text{Therefore,}$$

$x^3 + ax = b$ : but we know, that a near value of  $x$  is .0547: and, having corrected this value, we shall find,  $x = \sin \phi = .0540430$ ; therefore,  $\sin \phi = .0540430 = \sin 3^\circ 5' 52''$ , and  $\phi = 176^\circ 54' 8''$ .

As  $\psi$  is here a small angle, we must use the method of Art. 12. to find it with the requisite exactness: this gives  $\tan A =$

$$e \times \frac{\sin \phi}{\cos \frac{\phi - m}{2}} \times \sec 45^\circ, \text{ and } \sin \frac{\psi}{2} = \tan \frac{A}{2} \times \sin 45^\circ; \text{ there-}$$

fore  $\log. \tan A = 8.8689481$ , and  $\psi = 2^\circ 59' 32''$ ,

wherefore  $p' = \phi - \psi = 173^\circ 54' 36''$ , which is greater than the anomaly of the eccentric, reckoning from the aphelion, but very near to it: the error not exceeding three or four seconds. The eccentric anomaly, reckoned from the perihelion, is therefore nearly  $6^\circ 5' 24''$ , but greater than this arch.

16. IN this method of calculating, though any degree of accuracy may be obtained, yet when the distance from the perihelion is very small, the computation may run out to considerable length. In seeking a remedy for this inconvenience, I saw that much advantage might be obtained, by comparing the motion in an eccentric ellipse with the motion in a parabola, since at the perihelion they so nearly agree. The result of this comparison will perhaps be thought to make an useful addition to the general methods explained above.

LET there be proposed this geometrical problem: An eccentric ellipse being given, and likewise a parabola, having the same perihelion distance with the ellipse; it is required to draw a radius vector in the ellipse, to cut off a sector that shall be equal to a given sector of the parabola.

IF this problem can be resolved, the application of it to the present research will be easy.

LET the radii vectores that cut off the equal sectors from the ellipse and the parabola, be respectively denoted by  $\rho$  and  $r$ ; and let  $v$  and  $z$  be the angles that  $\rho$  and  $r$  make with the axes of the curves, reckoned from the perihelia: then, considering the two sectors as variable quantities, we shall have

$$\rho^2 v = r^2 z,$$

for these are obviously the doubles of the fluxions of the two sectors.

LET  $a$  be the mean distance of the ellipse, and  $\varepsilon$  the eccentricity: then, from the known property of the curve,

$$\rho = \frac{a(1 - \varepsilon^2)}{1 + \varepsilon \cos v}.$$

SUPPOSE

SUPPOSE  $x = \tan \frac{v}{2}$ ; then  $\cos v = \frac{1-x^2}{1+x^2}$ : and, if we put  $p$

to denote the perihelion distance,  $= a (1 - \epsilon)$ , and  $\lambda = \frac{1-\epsilon}{1+\epsilon}$ , we shall obtain, by substitution,

$$\epsilon = \frac{p(1+\epsilon)(1+x^2)}{(1+\epsilon) + (1-\epsilon)x^2} = p \times \frac{1+x^2}{1+\lambda x^2}.$$

Further, from the equation  $x = \tan \frac{v}{2}$ , we get,  $\dot{v} = \frac{2\dot{x}}{1+x^2}$ :

Therefore,

$$\epsilon^2 \dot{v} = 2 p^2 \times \frac{\dot{x}(1+x^2)}{(1+\lambda x^2)^2}.$$

AGAIN, the perihelion distance of the parabola being, by the supposition, equal to the perihelion distance of the ellipse, we have, from the nature of the curve,

$$r = \frac{2p}{1 + \cos z} = \frac{p}{\cos^2 \frac{z}{2}}.$$

LET  $y = \tan \frac{z}{2}$ : then  $\frac{1}{\cos^2 \frac{z}{2}} = 1 + y^2$  and  $z = \frac{2y}{1+y^2}$ : consequently,

$$r^2 \dot{z} = 2 p^2 \times \dot{y} (1 + y^2).$$

Now, equating the values of  $\epsilon^2 \dot{v}$  and  $r^2 \dot{z}$ , that have just been obtained, and, omitting the common multiplier,  $2 p^2$ , there will result,

$$\dot{y} (1 + y^2) = \frac{\dot{x}(1+x^2)}{(1+\lambda x^2)^2}.$$

and taking the fluents

$$y + \frac{y^3}{3} = \left\{ x + \frac{x^3}{3} \right\} - 2\lambda \times \left\{ \frac{x^3}{3} + \frac{x^5}{5} \right\} + 3\lambda^2 \left\{ \frac{x^5}{5} + \frac{x^7}{7} \right\} - 4\lambda^3 \left\{ \frac{x^7}{7} + \frac{x^9}{9} \right\} + \&c.$$

It is manifest, that this fluent requires no correction; because

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the two angles,  $v$  and  $z$ , are supposed to begin to flow together at the perihelia of the curves.

17. ACCORDING to the supposition, the eccentricity  $\epsilon$  is nearly equal to unity, and consequently,  $\lambda = \frac{1 - \epsilon}{1 + \epsilon}$  will be a small fraction: but, as  $x \left( = \tan \frac{v}{2} \right)$  increases from 0 to  $\infty$ , it is obvious, that the fluent, obtained above, will be of use in computation only to a certain limit, however small  $\lambda$  may be supposed to be. For the part of the fluent depending on  $\lambda$  manifestly converges by the powers of the quantity  $\lambda x^2$ ; and therefore, as long as  $\lambda x^2$  is a small fraction, so long only can we compute  $x$  when  $y$  is given, by means of the fluent: but when  $x$  has passed that limit, the fluent, in the form here given to it, is no longer of any use in computation.

BUT the fluent, although limited in its application by the consideration just explained, will enable us to compute  $x$  when  $y$  is given, and to determine the angle  $v$  of true anomaly in the ellipse, by means of the angle  $z$  in the parabola, for a considerable portion of the elliptic orbit lying adjacent to the perihelion, on either side. We may therefore deduce from it a series that will serve to compute the true place of a comet in the portion of its orbit which it describes during one apparition.

IN order to determine the angle  $v$  by means of the angle  $z$ , we must first find a value of  $x$  in terms of  $y$ : and, to avoid too complex calculations, we shall neglect the terms multiplied by the powers of  $\lambda$ , higher than the square. The extreme smallness of  $\lambda$ , in the orbits of all the comets, permits to simplify the calculation in this manner, and, nevertheless, to obtain a result, that will be sufficiently exact during the time of one apparition.

NEGLECTING, then, the terms multiplied by the powers of  $\lambda$  higher than the square, we have

$$y + \frac{y^3}{3} = \left\{ x + \frac{x^3}{3} \right\} - 2\lambda \left\{ \frac{x^3}{3} + \frac{x^5}{5} \right\} + 3\lambda^2 \left\{ \frac{x^5}{5} + \frac{x^7}{7} \right\}.$$

Assume



Assume  $x = y + A \lambda y^3 + B \lambda^2 y^5$ ,

A and B being indeterminate quantities, not depending on  $\lambda$ : Then, neglecting the quantities which the degree of exactness prescribed permits us to neglect, we shall find,

$$x^3 = y^3 + 3 A \lambda y^5 + 3 (B + A^2) \lambda^2 y^7,$$

$$x^5 = y^5 + 5 A \lambda y^7,$$

$$x^7 = y^7 + 7 A \lambda y^9 + 7 B \lambda^2 y^{11} + 7 A^2 \lambda^2 y^{11}.$$

If now we substitute these values in the equation between  $x$  and  $y$ , and omit the terms common to both sides, there will result,

$$0 = (A + A y^2) \times \lambda y^3 + (B + B y^2 + A^2 y^2) \times \lambda^2 y^5,$$

$$- 2 \left( \frac{1}{3} + \frac{1}{5} y^2 \right) \times \lambda y^3 - 2 (A + A y^2) \times \lambda^2 y^5,$$

$$+ 3 \left( \frac{1}{5} + \frac{1}{7} y^2 \right) \times \lambda^2 y^5.$$

Hence

$$A = \frac{2}{3} \times \frac{1}{1+y^2} + \frac{2}{5} \times \frac{y^2}{1+y^2};$$

$$B = \frac{11}{15} \times \frac{1}{1+y^2} + \frac{13}{35} \times \frac{y^2}{1+y^2} - A^2 \times \frac{y^2}{1+y^2}.$$

But, since  $y = \tan \frac{z}{2}$ ; therefore  $\frac{1}{1+y^2} = \cos^2 \frac{z}{2} = 1 - \sin^2 \frac{z}{2}$ ,

and  $\frac{y^2}{1+y^2} = \sin^2 \frac{z}{2}$ : consequently

$$A = \frac{2}{3} - \frac{4}{15} \sin^2 \frac{z}{2},$$

$$B = \frac{11}{15} - \frac{254}{315} \sin^2 \frac{z}{2} + \frac{16}{45} \sin^4 \frac{z}{2} - \frac{16}{225} \sin^6 \frac{z}{2}.$$

18. SUPPOSE  $v = z + w$ ;  $w$  expressing the difference of the two angles  $v$  and  $z$ , which, it is obvious, depends on  $\lambda$ , and is to be reckoned of the same order with that quantity: Then

$x = \tan \frac{v}{2} = \tan \frac{z+w}{2}$ : but  $y = \tan \frac{z}{2}$ ; therefore, according

to TAYLOR's theorem, rejecting the quantities that ought to be rejected,

rejected,

$$x = \tan \frac{z+w}{2} = y + \frac{\dot{y}}{z} \times w + \frac{1}{2} \cdot \frac{\ddot{y}}{z^2} \times w^2.$$

Assume now  $w = C \times \lambda + D \times \lambda^2$ ;  $C$  and  $D$  being indeterminate quantities, not depending on  $\lambda$ ; then, by substitution,

$$x = y + \frac{\dot{y}}{z} \times C \lambda + \left\{ \frac{\dot{y}}{z} D + \frac{1}{2} \cdot \frac{\ddot{y}}{z^2} \times C^2 \right\} \times \lambda^2.$$

But  $\frac{\dot{y}}{z} = \frac{1+y^2}{2}$ ; and  $\frac{1}{2} \cdot \frac{\ddot{y}}{z^2} = \frac{y(1+y^2)}{4}$ ; therefore,

$$x = y + \frac{C}{2} \times (1+y^2) \times \lambda + \left\{ \frac{D}{2} + \frac{C^2}{4} y \right\} \times (1+y^2) \times \lambda^2.$$

WE must now determine  $C$  and  $D$ , so that this value of  $x$ , and its value already found, may be identical: Thus we have,

$$\frac{C}{2} = A \times \frac{y^2}{1+y^2} \times y,$$

$$\frac{D}{2} + \frac{C^2}{4} \times y = B \times \frac{y^2}{1+y^2} \times y^3;$$

therefore,

$$C = 2 A \times \sin^2 \frac{z}{2} \times y,$$

$$D = \left\{ 2 B \sin^2 \frac{z}{2} - 2 A^2 \sin^4 \frac{z}{2} \right\} \times y^3,$$

and, taking the values of  $A$  and  $B$ , (Art. 17.)

$$C = \left\{ \frac{4}{3} \sin^2 \frac{z}{2} - \frac{8}{15} \sin^4 \frac{z}{2} \right\} \times y;$$

$$D = \left\{ \frac{22}{15} \sin^2 \frac{z}{2} - \frac{788}{315} \sin^4 \frac{z}{2} + \frac{64}{45} \sin^6 \frac{z}{2} - \frac{64}{225} \sin^8 \frac{z}{2} \right\} \times y^3,$$

and finally, if we substitute for the powers of  $\sin \frac{z}{2}$ , their values in the cofines of the multiples of the arch, we shall find,

$$C = \frac{1}{15} \times \{ 7 - 6 \operatorname{cof} z - \operatorname{cof} 2z \} \times y,$$

$$D = \frac{1}{3150} \{ 510 - 78 \operatorname{cof} z - 341 \operatorname{cof} 2z - 84 \operatorname{cof} 3z - 7 \operatorname{cof} 4z \} y^3$$

Having

Having thus found C and D, we have  $v = z + \dot{w} = z + C\lambda + D\lambda^2$ : that is,

$$v = z + \frac{1}{15} \{ 7 - 6 \cos z - \cos 2z \} \lambda \tan \frac{z}{2},$$

$$+ \frac{1}{3150} \{ 510 - 78 \cos z - 341 \cos 2z - 84 \cos 3z - 7 \cos 4z \} \lambda^2 \tan^3 \frac{z}{2}.$$

19. Having now resolved the problem that was proposed, it remains to apply it to find the true place of a comet in an eccentric orbit. For this purpose, nothing more is wanting, than to be able to determine the angle  $z$  in the parabola, at any given instant of time, reckoning from the passage over the perihelion. We shall here suppose, as a matter already known and demonstrated, the theory that is commonly given of a body describing a parabolic trajectory round the sun, placed in the focus: and we shall also make use of the astronomical tables that have been computed, for finding the true place in that trajectory when the time is given. It would indeed be easy for us to deduce the whole of that theory, and to explain the construction of the tables, from the fluxional equation,

$$r^2 \dot{z} = 2p^2 \times \dot{y} (1 + y^2)$$

obtained above: but this would only be to repeat what is already familiar to astronomers.

SUPPOSE, then, a body to describe the given parabola, by its gravitation to the sun placed in the focus; and let us compare the motion of the body in the parabola, with the motion of the comet in the eccentric orbit: If two bodies describe different conic sections by the action of a central force, tending to the foci of the curves, and varying inversely as the square of the distance, it is demonstrated, by the writers on central forces, (*Vide* NEWT. *Prin. Matb. lib. 1. prop. 14.*) that they will describe areas, in the same time, that are in the subduplicate ratio of the two parameters: Therefore, the area described by the body in the parabola, in any given time, will be to the area described

by

by the comet, in the same time as the square root of the parameter of the parabola to the square root of the parameter of the ellipse; that is, as  $\sqrt{2p}$  to  $\sqrt{a(1-e^2)}$ ; or as  $\sqrt{2p}$  to  $\sqrt{p(1+e)}$ ; or as  $\sqrt{2}$  to  $\sqrt{1+e}$ ; or, finally, as  $\sqrt{1+\lambda}$  to 1. But the area, described by the comet in the eccentric ellipse, that is, the sector of the ellipse cut off by the radius vector  $\rho$ , is equal to the sector of the parabola, cut off by the radius vector  $r$ : Therefore, the sector cut off by a radius vector drawn to the body, describing the parabola by the solar force, will be to the sector cut off by the radius vector  $r$ , in the proportion of  $\sqrt{1+\lambda}$  to 1. Now, in the table of the motion in a parabola, the arguments of the true anomaly are no other than the areas cut off by the radii vectores; or, which is the same thing, they are numbers proportional to those areas: Therefore, if the argument of the true anomaly of the body in the parabolic trajectory, found for the given instant of time, be diminished in the proportion of  $\sqrt{1+\lambda}$  to 1, we shall have the argument, which corresponds in the table, to the angle  $z$  required.

WE have, therefore, this rule for finding the angle  $z$  at any given time, by means of the table of the motion in a parabola\*: Let  $d$  denote the interval between the given time and the passage over the perihelion, expressed in days; then  $z$  will be the angle in the table that corresponds to the argument,  $\frac{d}{p^{\frac{1}{2}}} \times \frac{1}{\sqrt{1+\lambda}}$ .

Having thus found the angle  $z$  in the parabola for the given time, we must apply to it the *equation* in the formula of Art. 18. to have the true anomaly in the eccentric orbit†.

5. IN

\* *Vide* Table Generale du Mouvement des Cometes, Astronomie de LA LANDE, tom. iii. p. 335. 2d edit.

† It may be remarked, that the angle  $z$  is always less than the angle  $v$ , and that the equation to be applied to  $z$  is always additive.

20. IN calculating the place of a comet, as seen from the earth, the astronomer has occasion to compute, not only the heliocentric

If we compare the motion of the comet in the eccentric orbit, immediately to the motion of the body in the parabolic trajectory, it is obvious, that the angular velocity of the former is less than the angular velocity of the latter, at the perihelia of the curves: therefore, supposing the two bodies to pass over the perihelia together, the body in the parabola will advance before the comet. But, as the radii vectores in the ellipse increase at a slower rate than the radii vectores in the parabola, the angular velocity in the ellipse will increase at a faster rate than the angular velocity in the parabola, in order that the areas described in the same time may preserve their just proportion. Hence it is clear, that the angular velocity of the comet will, in the first place, become equal to the angular velocity of the body in the parabola, after which the former body will gain upon the latter; the difference of the true anomalies will become less and less, and will at last vanish, the two heliocentric places exactly coinciding.

If we denote by  $v$  the true anomaly, common to both the ellipse and parabola, when the heliocentric places coincide; and if  $x = \tan \frac{v}{2}$ , it will not be difficult to deduce, from the reasoning above, the following equation for determining  $x$ , viz.

$$\frac{1}{\sqrt{1+\lambda}} \times \left\{ x + \frac{x^3}{3} \right\} = \left\{ x + \frac{x^3}{3} \right\} - 2\lambda \left\{ \frac{x^3}{3} + \frac{x^5}{5} \right\} + 3\lambda^2 \left\{ \frac{x^5}{5} + \frac{x^7}{7} \right\} - \&c.$$

which is easily reduced to this,

$$0 = \frac{1}{\lambda} \times \left( 1 - \frac{1}{\sqrt{1+\lambda}} \right) \times \left\{ 1 + \frac{x^2}{3} \right\} - 2 \left\{ \frac{x^2}{3} + \frac{x^4}{5} \right\} + 3\lambda \left\{ \frac{x^4}{5} + \frac{x^6}{7} \right\} - \&c.$$

and, if we neglect the quantities multiplied by  $\lambda$  and its powers, we shall have simply,

$$0 = \frac{1}{2} - \frac{x^2}{2} - \frac{2x^4}{5},$$

whence  $x = \sqrt{\frac{105}{8}} = 0.8098 = \tan 39^\circ$ , nearly.

THEREFORE  $v = 78^\circ$ : and, whatever be the eccentricity of the orbit, provided it be very great, the heliocentric place in the ellipse will, at this distance from the perihelion, coincide with the heliocentric place in the parabola: nearer the perihelion, the true anomaly in the ellipse will be less than the true anomaly in the parabola: and, more remote from the perihelion, the true anomaly in the ellipse will be greater than the true anomaly in the parabola. I need not remark, that this conclusion is not to be understood with the utmost rigour; for we have arrived at it by neglecting the quantities multiplied by the small fraction  $\lambda$  and its powers.

tric place, but also the distance of the comet from the sun, or the radius vector of the orbit. The formula  $\rho = p \times \frac{1+x^2}{1+\lambda x^2}$  found in Art. 16. will furnish a convenient rule for this purpose :

For, if we make  $\tan u = x \times \sqrt{\lambda} = \tan \frac{v}{2} \times \sqrt{\lambda}$ ; we shall have

$\operatorname{cof}^2 u = \frac{1}{1+\lambda x^2}$ , and  $\frac{1}{\operatorname{cof}^2 \frac{v}{2}} = 1+x^2$ ; and, therefore, also

$\rho = \frac{\operatorname{cof}^2 u}{\operatorname{cof}^2 \frac{v}{2}} \times p$ , whence  $\rho$  may be found.

**X.**

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X. DESCRIPTION of *some IMPROVEMENTS in the ARMS and ACCOUTREMENTS of LIGHT CAVALRY, proposed by the Earl of ANCRAM, Colonel of the Mid-Lothian Regiment of Fencible Cavalry, and F. R. S. EDIN. to his Excellency Marquis CORNWALLIS, Lord-Lieutenant of Ireland, &c. &c. in a LETTER to Captain TAYLOR, Military Secretary to his Excellency.*

[*Read, March 4. 1799.*]

S I R, *Hillsbro' Dec. 23. 1798.*

SOME time in the month of July 1797, I had a carabine made at Drogheda, of different dimensions, and of a different construction from that of our Light Dragoons: the mode of carrying and of using it is likewise different; and, as I have had considerable experience of this carabine since that period, I take the liberty of recommending it to the notice of his Excellency the Lord Lieutenant, in order that a trial may be made, whether the adoption of such an *arm* might not be of advantage to the Light Cavalry.

THE barrel is twenty-one inches in length, (Pl. IV. fig. 1.) and of carabine bore. The weight of the carabine is not more than five pounds. The length of the bayonet-blade is thirteen inches, (fig. 2.) and there is a spring on the ring of it, to prevent the possibility of its flying off the barrel. The touch-hole is of a conical shape; the base of the cone opening into the pan, the summit into the barrel. The breech-plug is made to slope down towards the touch-hole, so that the carabine primes itself, the powder running

ning into the pan by means of two inclined planes, the one formed by the shape of the breech-plug, the other by that of the touch-hole. The butt is excavated, so as to admit the hand easily, and it is strengthened by a plate of iron, which runs from the end of the breech-plug, round to the end of the steel-tube which holds the ramrod.

THE advantages which it possesses over the common carabine, are these: It is stronger, because it is fortified all round with iron; it is upwards of two pounds lighter; and, although seven inches shorter in the barrel, it carries as far as the other.

ONE might expect, that as the touch-hole is somewhat larger than usual, there must be a greater explosion through it, and consequently a diminution of the explosion through the barrel, as well as of the velocity of the bullet; but the fact is, that the touch-hole, although it be larger at the base, it is not much larger at the summit, where it opens into the barrel, than the common touch-hole; and, by frequent and accurate experiment, I have found, that, with the same quantity of powder, the small carabine throws a ball with a force fully equal, if not superior, to that of the common carabine. Perhaps the circumstance of the length of the barrel, corresponding better with the diameter of the bullet, may account for this fact, and likewise make up for any trifling diminution of the explosion from the barrel, which may possibly be occasioned by the somewhat increased size of the touch-hole.

As this carabine primes itself, it can be loaded on horse-back, at speed, or in the dark, or in windy weather, and under the cloak when it rains. I have never known it to miss, priming; and, by way of experiment, I have fired it upwards of twenty times successively, and have always found, on opening the pan, that it was as well primed at the last, as at the first shot. It can be fired easily five times in a minute, with the assistance of a second ramrod, (fig. 3.) which,

when



when the dragoon is mounted, hangs to his carabine-belt, or to a button placed below the right shoulder. By means of the iron-plate, and the excavation in the butt, the dragoon is enabled to act on the defensive against the sabre, by parrying, in case of necessity, with the usual protects established in the sword-exercise, and, with the bayonet fixed, he is likewise enabled to act on the offensive, by giving point, as well against infantry as cavalry. The carabine is also used with so much ease and accuracy as a pistol, as to supersede the necessity of encumbering the dragoon with one. When he dismounts, it is thrown over the left shoulder, slung by means of a ring fastened to the handle of the butt; and the muzzle is slipped into a small leathern bucket, attached to the belt, a little above, and behind the right hip-bone. In this position, it is carried without impeding the movements of the sword-arm.

It is to be observed, that a powder somewhat finer granulated than the common Government powder, is required for the use of this carabine; and I have found, on inquiry, that the expence of granulation is a mere trifle.

THE carabine is placed with the muzzle downwards, behind the right thigh of the dragoon, in a long holster, (fig. 4.) which reaches above the lock and protects it, and which is fastened at the bottom to a girth, by means of a ring and strap, and at the top by a loop to the back-part of the saddle. It might be fastened on before, in the manner in which dragoons usually fix their buckets, but, by the above method, it is not only out of the way of the action of the sword-arm, but it is of no more encumbrance to him than the pistol, and can be drawn and returned with greater facility, there being no flounce to the holster, which is trumpet-mouthed, in order to render this operation the easier when performed at speed. The hammer is likewise raised as high as the top of the cock, as a guard, in order to prevent the possibility of the piece cocking itself, which so often happens to  
the

the pistol when suddenly returned, and to the carabine or musket, when brushing through an hedge or other inclosure. The facility of drawing and returning the carabine, also renders the use of the carabine belt and swivel to the dragoon when mounted unnecessary; a matter of no small importance, as the carabine when slung almost disables the sword-arm from acting. A small priming flask, (fig. 5.) containing about two ounces of powder, made of horn, which hangs from the left shoulder below the right elbow, will be found of great convenience, by saving the breaking of a fresh cartridge, if in windy or wet weather the piece should happen to miss fire.

I HAVE likewise to add, that, owing to the number of accidents which troop horses are subject to, from the weight, inconvenience, bad construction, and insufficiency of our dragoon saddle, I have made some alteration on the common hussar-saddle, which, I cannot help flattering myself, corrects, in a great measure, the defects of both. The common hussar-saddle is shorter than ours; it raises the seat of the rider considerably above the horse's back. The stirrup-leathers are likewise removed further back. Owing to these circumstances, the hussar sits very high and erect on his horse; he appears rather to rest on his fork than on his seat, his balance inclining forward. The principal advantages of this saddle are, that it fits all horses equally well, and that the back-bone of the animal is perfectly free and untouched. The disadvantages of it are, that, by placing the horseman so high above the horse, the security of his seat is affected, because, the nearer a man sits to his horse's back, the firmer is the grasp which he is able to make with his thighs, and the less likely he is to lose his balance; and although in a flat open country the inconvenience of the hussar-seat may not be so much felt, yet in a hilly or inclosed one, and where leaping is necessary, the rider is continually exposed to be thrown forward on his horse's neck; besides which, there is

a considerable degree of trouble and time required in saddling, for, after the saddle is put on, which is little more than a tree, the sheep-skin or cloth is to be thrown over and fastened.

THE alteration I have made consists in removing the cantle about two inches back, and the stirrups one inch forward; by depressing the seat to within one inch of the horse's back, and by making it somewhat broader. By this means there is still a current of air running below the saddle, the dragoon still retains the manege-seat, and all the material advantages of the hussar-saddle are preserved. The saddle is covered with leather; it is infinitely stronger, possesses every useful property of our dragoon-saddle, and weighs five pounds less.

IN general, I cannot help observing, that the sabre ought to be considered as the right-arm of light cavalry, the carbine or pistol merely as an auxiliary, to be used when the sword is rendered useless, or in a woody, morassy, or inclosed country, where a light horseman is unable to come to close quarters with his adversary. A light dragoon cannot be too lightly clad, armed and accoutred; for, as activity and expedition are his principal qualifications, he must not be encumbered by any unnecessary weight. A light dragoon and his horse should be considered as one animal: his arms must be such as he can use with ease and dexterity when mounted; at the same time, provided it can be done without any additional encumbrance or inconvenience, there is no reason why they should not be adapted as much as possible to his defence when dismounted; but, if you attempt to make him equal at the same time to an hussar, and to an infantry man, you inevitably render him inferior to either.

WITH the carbine described above, I think the pistol totally unnecessary. A dragoon can only load and fire one *arm* at a time in action; another is superfluous; it only gives him additional trouble, and his horse additional weight; besides which,  
when

when he dismounts, he is obliged to leave his pistol in his holster, so that one half of his arms remains with his horse.

I CONFESS I am sanguine enough to conceive, that this *arm* might be in general use for the light cavalry, or at least, that to every regiment of cavalry, both light and heavy, according to its strength, a light squadron or troop should be attached, armed as above, to act relatively to the rest of the regiment, as the light infantry do to the battalion companies; and who should be constantly exercised in all detached duties, such as are performed by skirmishers or flankers, scouts, vedettes, &c. and who, by being thus lightly armed and accoutred, are better fitted for the alert and active operations which are required in these services.

I have the honour to be,

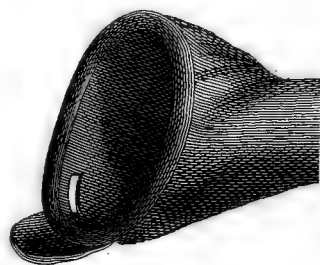
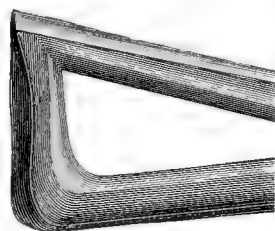
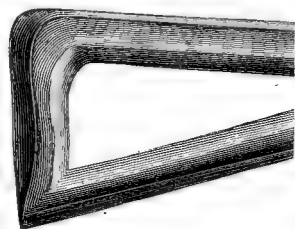
S I R,

&c. &c. &c.

ANCRAM,  
Col. M. L. F. C.

*To Captain Taylor,  
Military Secretary to his Excellency  
the Lord-Lieutenant, &c. &c.  
Dublin Castle.*

*Note.* SINCE writing the above, I have found it more convenient to remove the holster and carabine from behind the right thigh, to the fore-part of the saddle, where a dragoon usually fixes his bucket, having observed, that at close files, the holsters are, in the former position, more liable to be rubbed and injured.



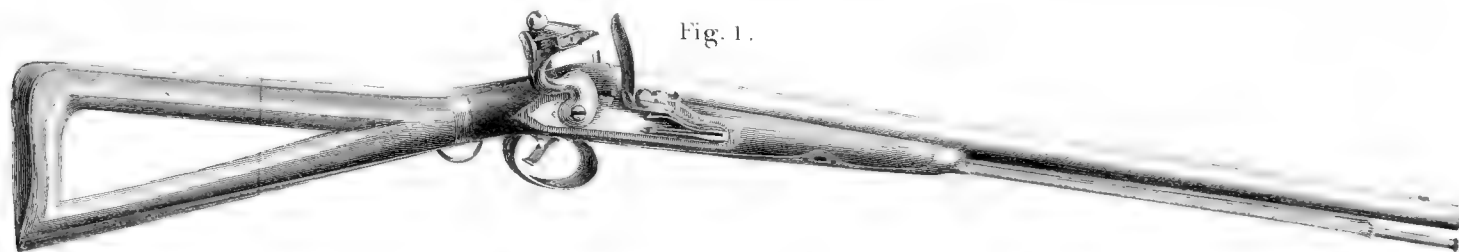


Fig. 1.

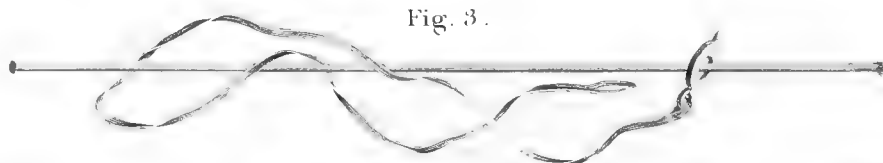


Fig. 3.

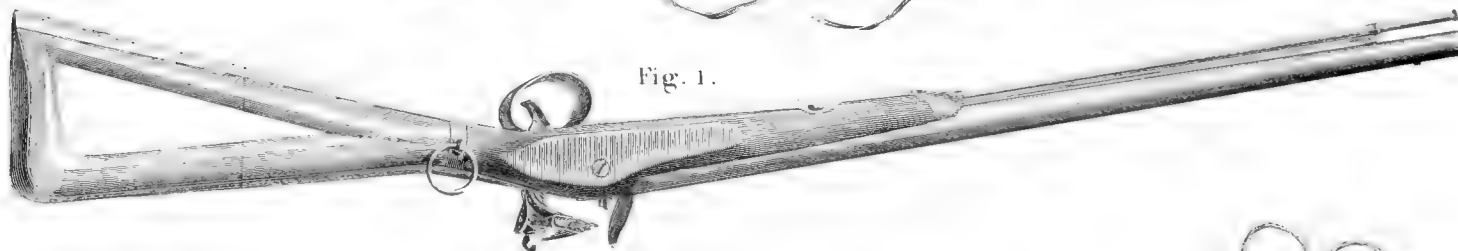


Fig. 1.

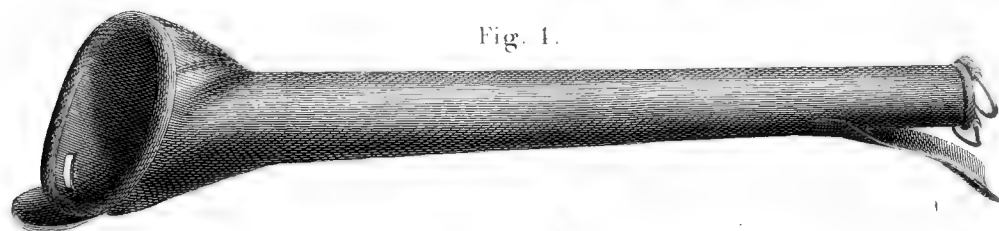


Fig. 1.

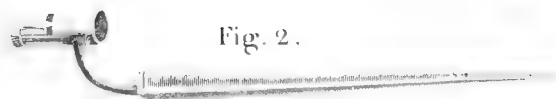


Fig. 2.



Fig. 5.

Scale of  $\frac{1}{2}$  1 2 3 4 5 6 7 8 9 10 11 12  
Inches.

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XI. A NEW METHOD of expressing the COEFFICIENTS of the DEVELOPMENT of the ALGEBRAIC FORMULA  $(a^2 + b^2 - 2ab \cos \phi)^n$ , by means of the Perimeters of two Ellipses, when  $n$  denotes the Half of any Odd Number; together with an APPENDIX, containing the INVESTIGATION of a FORMULA for the RECTIFICATION of any ARCH of an ELLIPSE. By Mr WILLIAM WALLACE, Assistant-Teacher of the Mathematics in the Academy of Perth.

[Read Jan. 11. 1802.]

I. IN calculating the effect of the mutual action of two planets upon each other, it has been found necessary to develop the algebraic formula  $(a^2 + b^2 - 2ab \cos \phi)^n$  into a series of this form,  $A + B \cos \phi + C \cos 2\phi + D \cos 3\phi + \&c.$  Here  $a$  and  $b$  denote the distances of the planets from the sun;  $\phi$  denotes the angle of commutation; and the values of  $n$ , more immediately the subject of consideration, are  $-\frac{3}{2}$ , and  $-\frac{5}{2}$ .

THE determination of the coefficients  $A, B, C, \&c.$  in these cases, appears to have been considered as a matter of difficulty by the mathematicians who first applied to the solution of the problem; for they found, that although it was only necessary to compute the first two coefficients  $A$  and  $B$ , the rest being easily derived from them, yet it did not appear that they could be expressed in finite terms, nor even by means of circular arches, or by logarithms. Recourse was therefore had to other methods, and chiefly to the method of infinite series; but

as the series which most readily occurred to them, converged in some cases so slowly as to be in a manner useless, no small degree of analytical address has been found necessary, either to render it more convergent, or to find the sum of a competent number of its terms, with a moderate degree of labour.

2. BUT in considering the subject, it has occurred to me, that although we cannot express the values of the coefficients in finite algebraic terms, nor even by means of circular arches, or by logarithms, yet when  $n$  is the half of an odd number, either positive or negative, we may always express them by means of the proportion which the perimeters, or semi-perimeters, of two ellipses bear to those of their circumscribing circles. The problem may therefore be reduced to the rectification of the circle and ellipse, and mathematicians know that such reduction is considered as the next degree of resolution, in point of simplicity, to our being able to effect the solution by means of circular arches, or by logarithms only.

3. IT is well known that we can easily obtain a fluxionary expression for each of the coefficients  $A, B, C, \&c.$  in the equation  $(a^2 + b^2 - 2ab \cos \phi)^n = A + B \cos \phi + C \cos 2\phi + D \cos 3\phi + \&c.$ ; for if each side of the equation be multiplied by  $\dot{\phi}$ , and the fluent taken, we get

$$\int \dot{\phi} (a^2 + b^2 - 2ab \cos \phi)^n = A\phi + B \sin \phi + \frac{1}{2}C \sin 2\phi + \frac{1}{3}D \sin 3\phi + \&c.$$

LET us now take the fluent generated, while  $\phi$  from 0 becomes a semicircle, then  $\sin \phi, \sin 2\phi, \sin 3\phi, \&c.$  all vanish; so that, putting  $\pi$  to denote half the perimeter of a circle, of which the radius is 1, we get,

$$\pi A = \int \dot{\phi} (a^2 + b^2 - 2ab \cos \phi)^n.$$

IN like manner, if we multiply each side of the assumed equation



equation by  $2\dot{\phi} \cos \phi$ , and take the general equation of the fluents, we get

$$\int 2\dot{\phi} \cos \phi (a^2 + b^2 - 2ab \cos \phi)^n = B\phi + (2A + C)\sin \phi + \frac{1}{2}(B + D)\sin 2\phi + \&c.;$$

therefore, when  $\phi = \pi$ ,

$$\pi B = \int 2\dot{\phi} \cos \phi (a^2 + b^2 - 2ab \cos \phi)^n.$$

Again, if we multiply both sides of the same equation by  $2\dot{\phi} \cos 2\phi$ , and take the relation of the fluents as before, we get

$$\pi C = \int 2\dot{\phi} \cos 2\phi (a^2 + b^2 - 2ab \cos \phi)^n.$$

4. PROCEEDING in this way, we might get a fluxionary expression for each of the remaining coefficients D, E, &c. but this is not necessary, for they may be all found from the first two, A and B, and from one another, as we have already observed; and the scale of their relation has been determined as follows \* :

LET the fluxions of the logarithms of each side of the assumed equation

$$(a^2 + b^2 - 2ab \cos \phi)^n = A + B \cos \phi + C \cos 2\phi + \&c.$$

be taken, and the whole be divided by  $\dot{\phi}$ ; also, for the sake of brevity, let us put  $\Delta$  for  $\frac{a^2 + b^2}{ab}$ ; thus we get

$$\frac{-2n \sin \phi}{\Delta - 2 \cos \phi} = \frac{B \sin \phi + 2C \sin 2\phi + 3D \sin 3\phi + \&c.}{A + B \cos \phi + C \cos 2\phi + D \cos 3\phi + \&c.}$$

LET the numerator of each side of this equation be now multiplied by the denominator of the other side, substituting  $\sin(p+1)\phi + \sin(p-1)\phi$  for  $2 \cos \phi \sin p\phi$ , and,  $\sin(p+1)\phi - \sin(p-1)\phi$  for  $2 \sin \phi \cos p\phi$ , then, putting the result = 0, we get

$$K k 2 \qquad \qquad \qquad + \Delta B$$

\* *Traité du Calcul Différentiel et du Calcul Integral, par LACROIX; vol. ii. page 120.*

$$\left. \begin{array}{l} + \Delta B \\ - 2C \\ + 2nA \\ - nC \end{array} \right\} \sin \varphi \left. \begin{array}{l} + 2\Delta C \\ - B \\ - 3D \\ + nB \\ - nD \end{array} \right\} \sin 2\varphi \left. \begin{array}{l} + 3\Delta D \\ - 2C \\ - 4E \\ + nC \\ - nE \end{array} \right\} \sin 3\varphi \left. \begin{array}{l} + 4\Delta E \\ - 3D \\ - 5F \\ + nD \\ - nF \end{array} \right\} \sin 4\varphi \&c. = 0;$$

hence we readily derive the following series of equations :

$$C = \frac{\Delta}{n+2} B + \frac{2n}{n+2} A$$

$$D = \frac{2\Delta}{n+3} C + \frac{n-1}{n+3} B$$

$$E = \frac{3\Delta}{n+4} D + \frac{n-2}{n+4} C$$

$$F = \frac{4\Delta}{n+5} E + \frac{n-3}{n+5} D$$

$$\&c. \qquad \&c.$$

5. BUT besides being able to determine the remaining coefficients of the development of the formula  $(a^2 + b^2 - 2ab \cos \varphi)_n$ , where the exponent is  $n$ , by means of the first two A and B, we can also determine all the coefficients of the development of  $(a^2 + b^2 - 2ab \cos \varphi)^{n-1}$ , where the exponent is  $n-1$ , by means of the same two coefficients A and B; and by them we can determine the coefficients when the exponent is  $n \pm q$ , where  $q$  denotes any whole number whatsoever\*.

FOR let us assume

$$(a^2 + b^2 - 2ab \cos \varphi)^{n-1} = A' + B' \cos \varphi + C' \cos 2\varphi + D' \cos 3\varphi + \&c.$$

Then,

$$(a^2 + b^2 - 2ab \cos \varphi)^n \begin{cases} = (a^2 + b^2 - 2ab \cos \varphi)(A' + B' \cos \varphi + C' \cos 2\varphi + \&c.) \\ = A + B \cos \varphi + C \cos 2\varphi + D \cos 3\varphi + \&c. \end{cases}$$

From these two values of  $(a^2 + b^2 - 2ab \cos \varphi)^n$ , by due reduction, and

\* *Traité du Calcul Differentiel et du Calcul Integral, par Lacroix; vol. ii. page 120.*

and substitution of  $\Delta$  for  $\frac{a^2+b^2}{ab}$ , and of  $\cos(p+1)\phi + \cos(p-1)\phi$  for  $2 \cos \phi \cos p\phi$ , we get the following equation,

$$\left. \begin{array}{l} \Delta A' + \Delta B' \\ - 2A' \\ - B' - C' \\ - \frac{A}{ab} - \frac{B}{ab} \end{array} \right\} \cos \phi \left. \begin{array}{l} + \Delta C' \\ - B' \\ - D' \\ - \frac{C}{ab} \end{array} \right\} \cos 2\phi \left. \begin{array}{l} + \Delta D' \\ - C' \\ - E' \\ - \frac{D}{ab} \end{array} \right\} \cos 3\phi, \&c. = 0.$$

HENCE it follows, that,

$$B' = \Delta A' - \frac{A}{ab}$$

$$C' = \Delta B' - 2A' - \frac{B}{ab}$$

$$D' = \Delta C' - B - \frac{C}{ab}$$

$$E' = \Delta D' - C - \frac{D}{ab}$$

&c. &c.

It yet remains to determine  $A'$ , the first coefficient; but that, as well as  $B'$ , may be readily found by considering that we have already found (Art 4.)

$$C = \frac{\Delta}{n+2} B + \frac{2n}{n+2} A;$$

therefore, substituting  $n-1$  for  $n$ , and  $C'$  for  $C$ , for the very same reason we get

$$C' = \frac{\Delta}{n+1} B' + \frac{2n-2}{n+1} A'.$$

By comparing together this value of  $C'$ , and the values of  $B'$  and  $C'$  as given in the last series of equations, and substituting for  $\Delta$  its value  $\frac{a^2+b^2}{ab}$ , we get,

$$A' = \frac{a^2+b^2}{(a^2-b^2)^2} A + \frac{(n+1)ab}{n(a^2-b^2)^2} B$$

$$B' = \frac{4ab}{(a^2-b^2)^2} A + \frac{(n+1)(a^2+b^2)}{n(a^2-b^2)^2} B.$$

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6. It is obvious, that if we had supposed  $A'$  and  $B'$ , the first two coefficients of the series  $A' + B' \cos \phi + C' \cos 2\phi + \&c.$  known, we could thence have determined  $A$  and  $B$  the first two coefficients of the series  $A + B \cos \phi + C \cos 2\phi + \&c.$ ; so that, in either case, the remaining coefficients  $C', D', \&c.$   $C, D, \&c.$  which depend upon the first two in each series, would also be known.

HENCE it follows, that while on the one hand we can proceed from the case in which the exponent is  $n$ , to those cases in which the exponents are  $n - 1, n - 2, \&c.$  so, on the other hand, we can proceed from the same case, to others in which the exponents are  $n + 1, n + 2$ , and thus we may go on, according to a descending or ascending scale, as far as we please.

7. I NOW proceed to the chief object of inquiry in this paper, namely, to find convenient geometrical expressions for the first two coefficients  $A$  and  $B$ , in some particular case, where the exponent  $n$  is the half of an odd number; and I select that in which  $n = -\frac{3}{2}$ , because of its importance in physical astronomy; but it is evident from what has been already shewn, that from hence we may determine the coefficients, in the cases of  $n = -\frac{5}{2}$ ,  $\&c.$  and also  $n = -\frac{1}{2}, n = +\frac{1}{2}, \&c.$

LET us therefore assume this equation,

$$(a^2 + b^2 - 2ab \cos \phi)^{-\frac{1}{2}} = A + B \cos \phi + C \cos 2\phi + \&c.;$$

or, putting, for the sake of brevity,  $\epsilon = \frac{b}{a}$ , where  $b$  is supposed to be less than  $a$ ,

$$(1 + \epsilon^2 - 2\epsilon \cos \phi)^{-\frac{1}{2}} = a^3 (A + B \cos \phi + C \cos 2\phi + \&c.)$$

Then, from what has been already shewn, (Art 3.) we have

$$\pi a^3 A$$

$$\pi a^3 A = \int \frac{\dot{\phi}}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}};$$

$$\pi a^3 B = \int \frac{2\dot{\phi} \cos \phi}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}};$$

the fluents to be considered as generated, while  $\phi$  increases from 0 to  $\pi$ .

8. LET us assume  $\psi$  such an arch, that

$$\sin \psi = \frac{\sin \phi}{\sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}};$$

from this assumption it is evident that when  $\phi = 0$ , then  $\psi = 0$ , and when  $\phi = \pi$ , then  $\psi$  also  $= \pi$ .

TAKING now the fluxion of each side of our assumed equation, we get

$$\dot{\psi} \cos \psi = \frac{\dot{\phi} \cos \phi}{\sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}} - \frac{\varepsilon \dot{\phi} \sin^2 \phi}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}},$$

and by reducing the fractional quantities to a common denominator, and substituting  $1 - \cos^2 \phi$  for  $\sin^2 \phi$ , the same equation becomes

$$\dot{\psi} \cos \psi = \frac{\dot{\phi}(1 - \varepsilon \cos \phi)(\cos \phi - \varepsilon)}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}}.$$

BUT  $(1 - \varepsilon \cos \phi)(\cos \phi - \varepsilon)$ , the numerator of the latter side of the equation, may be otherwise expressed, thus,

$\frac{1}{4\varepsilon} \{(\dot{1} - \varepsilon^2)^2 - (1 + \varepsilon^2 - 2\varepsilon \cos \phi)^2\}$ ; hence it follows that

$$\dot{\psi} \cos \psi \left\{ \begin{aligned} &= \frac{\dot{\phi} \{(\dot{1} - \varepsilon^2)^2 - (1 + \varepsilon^2 - 2\varepsilon \cos \phi)^2\}}{4\varepsilon(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}} \\ &= \frac{(\dot{1} - \varepsilon^2)^2 \dot{\phi}}{4\varepsilon(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}} - \frac{1}{4\varepsilon} \dot{\phi} \sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}; \end{aligned} \right.$$

therefore,  $\frac{\dot{\phi}}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}} = \frac{4\varepsilon \dot{\psi} \cos \psi}{(\dot{1} - \varepsilon^2)^2} + \frac{1}{(\dot{1} - \varepsilon^2)^2} \dot{\phi} \sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}$ ,

and, taking the fluents when  $\phi = \pi$ ,

$$\pi a^3 A = \frac{1}{(\dot{1} - \varepsilon^2)^2} \int \dot{\phi} \sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}.$$

LET

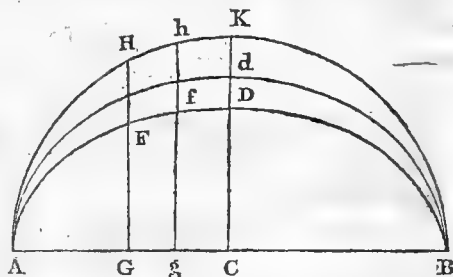
LET us now substitute  $2 \cos \frac{\phi}{2} - 1$ , for its equal,  $\cos \phi$ , and the fluxionary expression  $\dot{\phi} \sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}$  becomes

$(1 + \varepsilon) \dot{\phi} \sqrt{1 - \frac{4\varepsilon}{(1 + \varepsilon)^2} \cos \frac{\phi}{2}}$ ; so that if we assume  $e$  such, that  $e^2 = \frac{4\varepsilon}{(1 + \varepsilon)^2}$ , we also get

$$\pi a^3 A = \frac{1}{(1 + \varepsilon)(1 - e)^2} \int \dot{\phi} \sqrt{1 - e^2 \cos \frac{\phi}{2}}.$$

LET ADB be an ellipse, of which the semi-transverse axis  $AC = 1$ , and excentricity  $= e$ , and let AKB be a circle, having AB, the transverse axis, for its diameter; let FG, any ordinate to the transverse axis, meet the circle in H; then, if we put the arch  $AH = \frac{\phi}{2}$ , it is well known

that  $\frac{1}{2} \dot{\phi} \sqrt{1 - e^2 \cos \frac{\phi}{2}}$  will denote the fluxion of the elliptic arch AF; therefore, when  $\phi = \pi$ , the fluent of  $\dot{\phi} \sqrt{1 - e^2 \cos \frac{\phi}{2}}$  will evident-



ly be expressed by ADB, the semi-perimeter of the ellipse: Let us denote the semi-perimeter by (E) and we have  $a^3 \pi A = \frac{(E)}{(1 + \varepsilon)(1 - e)^2}$ ; hence, and by substituting  $\frac{b}{a}$  for  $\varepsilon$ , we get

$$A = \frac{1}{(a + b)(a - b)^2} \times \frac{(E)}{\pi};$$

and because  $e$ , the excentricity of the ellipse, has been assumed  $= \frac{2\sqrt{\varepsilon}}{1 + \varepsilon}$ , and  $\varepsilon = \frac{b}{a}$ ; therefore the excentricity  $= \frac{\pi\sqrt{ab}}{a + b}$ ; and the semi-conjugate axis  $= \frac{a - b}{a + b}$ .

9. HAVING now found a geometrical expression, sufficiently simple, for A, I proceed to investigate a similar expression for the

the next coefficient B; which, as we have already seen (Art. 7.)

is equal to  $\frac{1}{\pi a^3} \int \frac{2\phi \cos \phi}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}}$ .

FROM our assumed equation,  $\sin \psi = \frac{\sin \phi}{\sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}}$ , we have found  $\dot{\psi} \cos \psi = \frac{\dot{\phi}(1 - \varepsilon \cos \phi)(\cos \phi - \varepsilon)}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}}$ , (Art. 8.); but, since from

the same equation it appears that

$$\cos \psi = \sqrt{1 - \sin^2 \psi} = \frac{\cos \phi - \varepsilon}{\sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}};$$

$$\text{therefore, } \frac{\dot{\psi}(\cos \phi - \varepsilon)}{\sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}} = \frac{\dot{\phi}(1 - \varepsilon \cos \phi)(\cos \phi - \varepsilon)}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}};$$

and, dividing both sides by  $\frac{\cos \phi - \varepsilon}{\sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}}$ ,

$$\dot{\psi} = \frac{\dot{\phi}(1 - \varepsilon \cos \phi)}{1 + \varepsilon^2 - 2\varepsilon \cos \phi}. \quad (\alpha)$$

AGAIN, from our assumed equation, we have

$$\varepsilon^2 \sin^2 \psi = \frac{\varepsilon^2(1 - \cos^2 \phi)}{1 + \varepsilon^2 - 2\varepsilon \cos \phi}, \text{ and therefore,}$$

$$\sqrt{1 - \varepsilon^2 \sin^2 \psi} = \frac{1 - \varepsilon \cos \phi}{\sqrt{1 + \varepsilon^2 - 2\varepsilon \cos \phi}}. \quad (\beta)$$

LET the product of the corresponding sides of the equations ( $\alpha$ ) and ( $\beta$ ) be taken, and we get

$$\dot{\psi} \sqrt{1 - \varepsilon^2 \sin^2 \psi} = \frac{\dot{\phi}(1 - \varepsilon \cos \phi)^2}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}}.$$

Now we have found, (Art. 8.)  $\varepsilon \dot{\psi} \cos \psi = \frac{\dot{\phi}(1 - \varepsilon \cos \phi)(\varepsilon \cos \phi - \varepsilon^2)}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}}$ ,

therefore, taking the sum of these two equations, it follows that

$$\dot{\psi} \sqrt{1 - \varepsilon^2 \sin^2 \psi} + \varepsilon \dot{\psi} \cos \psi \begin{cases} = \frac{(1 - \varepsilon^2) \dot{\phi}(1 - \varepsilon \cos \phi)}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}} \\ = \frac{(1 - \varepsilon^2) \dot{\phi}}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}} - \frac{\varepsilon(1 - \varepsilon^2) \dot{\phi} \cos \phi}{(1 + \varepsilon^2 - 2\varepsilon \cos \phi)^{\frac{3}{2}}} \end{cases}$$

TAKING now the fluents, when  $\psi$  and  $\phi = \pi$ , and substituting  $\pi a^3 A$  for  $\int \frac{\dot{\phi}}{(1 + \epsilon^2 - 2\epsilon \cos \phi)^{\frac{3}{2}}}$ , also  $\frac{1}{2} \pi a^3 B$  for  $\int \frac{\dot{\phi} \cos \phi}{(1 + \epsilon^2 - 2\epsilon \cos \phi)^{\frac{3}{2}}}$ , we get,  $\int \psi \sqrt{1 - \epsilon^2 \sin^2 \psi} = (1 - \epsilon^2) \pi a^3 A - \frac{1}{2} \epsilon (1 - \epsilon^2) \pi a^3 B$ .

LET  $AdB$  be another ellipse, having its semi-transverse also  $= 1$ , but its excentricity  $= \epsilon$ ; let its conjugate axis meet the circle in  $K$ , and let  $gf$ , an ordinate to the transverse axis, meet the circle in  $b$ ; then, if the arch  $Kb$  be denoted by  $\psi$ , the fluxion of the elliptic arch  $df$  will be expressed by  $\dot{\psi} \sqrt{1 - \epsilon^2 \sin^2 \psi}$ ; therefore, when  $\psi = \pi$ , we have  $\int \dot{\psi} \sqrt{1 - \epsilon^2 \sin^2 \psi}$  equal to  $AdB$ , half the perimeter of the ellipse. Let us put  $(E')$  for the semi-perimeter of this second ellipse, and our last equation becomes

$$(E') = (1 - \epsilon^2) \pi a^3 A - \frac{1}{2} \epsilon (1 - \epsilon^2) \pi a^3 B,$$

and, by proper reduction, and substitution of  $\frac{b}{a}$  for  $\epsilon$ , we finally get

$$B = \frac{2a}{b} A - \frac{2}{b(a^2 - b^2)} \times \frac{(E')}{\pi};$$

and, since the excentricity of this other ellipse is  $\frac{b}{a}$ , its semi-conjugate axis  $= \frac{\sqrt{a^2 - b^2}}{a}$ .

THUS we have reduced the determination of  $A$  and  $B$ , the two first coefficients of the series, and upon which all the remaining coefficients depend, to the rectification of the ellipse: now this is a problem which we can readily resolve, by means of infinite series, in every case that can possibly occur.

10. I NEXT observe, that in determining the coefficients  $A$ ,  $B$ , &c. we are not confined to the two ellipses just now investigated; for, instead of them, we may substitute other two, having their excentricities as great or as small as we please. This peculiarity of our solution depends upon a very curious relation which



which is known to subsist between indefinite arches, as well as between the whole perimeters of any three contiguous terms of a series formed by an infinite number of ellipses, the axes, or excentricities of which have among themselves a very remarkable connection.

LET  $E, E', E'', E''', \&c.$  denote the semi-perimeters of a series of ellipses;  $e, e', e'', e''', \&c.$  their excentricities, and  $c, c', c'', c''', \&c.$  their conjugate axes; let these ellipses be so related to each other, that  $e' = \frac{1-c}{1+c} = \frac{1-\sqrt{1-e^2}}{1+\sqrt{1-e^2}}, e'' = \frac{1-c'}{1+c'} = \frac{1-\sqrt{1-e'^2}}{1+\sqrt{1-e'^2}},$   
 $e''' = \frac{1-c''}{1+c''} = \frac{1-\sqrt{1-e''^2}}{1+\sqrt{1-e''^2}}, \&c.$  Then we have the following series of equations\*.

$$(1 + e') E - (2 + c') E' + c' (1 + e') E'' = 0,$$

$$(1 + e'') E' - (2 + c'') E'' + c'' (1 + e'') E''' = 0,$$

$$(1 + e''') E'' - (2 + c''') E''' + c''' (1 + e''') E^{IV} = 0,$$

$$\&c. \qquad \qquad \&c. \qquad \qquad \&c.$$

THIS series of equations may be continued backwards by putting  $E'', E', \&c.$  for the semi-perimeters of ellipses of which the excentricities are  $e', e'', \&c.$  and semi-conjugate axes  $c', c'', \&c.$

and since  $e = \frac{1-\sqrt{1-e'^2}}{1+\sqrt{1-e'^2}}, e' = \frac{1-\sqrt{1-e''^2}}{1+\sqrt{1-e''^2}}, \&c.;$  therefore,  $e =$

$$\frac{2\sqrt{e'}}{1+e'}, e' = \frac{2\sqrt{e''}}{1+e''}, \&c.$$

11. FROM the foregoing series of equations, it appears, that any ellipse of the series  $E'', E', E, E', \&c.$  may be expressed by means of any other two ellipses of the same series: for the number of equations is always two less than the number of ellipses; and therefore, having assumed any number of equations, we

L 1 2. can,

\* SEE a Memoir upon the Comparison of Elliptic Arcs, by LEGENDRE, in the Memoirs of the Royal Academy of Sciences for 1786. See also the Appendix to this Paper.

can, by means of them, exterminate all the ellipses, except three, and thus obtain a single equation, expressing a relation between any three ellipses we may choose to retain.

Now, in considering the series of quantities  $e''$ ,  $e'$ ,  $e$ ,  $e'$ ,  $e''$ , &c. which denote the excentricities of the ellipses, it will readily appear, that in continuing the series forward, they rapidly diminish; for since  $e' = \frac{1-c}{1+c} = \frac{1-c^2}{(1+c)^2} = \frac{e^2}{(1+c)^2}$ , it is evident, that however small the conjugate axis of any one of the ellipses may be, the excentricity of the next ellipse in the series will be expressed by a fraction less than the square of that which expressed the excentricity of the former; but if the conjugate axis be nearly equal to the transverse, then the excentricity of the next ellipse is only a little greater than one-fourth of the square of the excentricity of the former ellipse.

LET us suppose, for example,  $e = .5$ , then it follows that  $e' = .071797$ ,  $e'' = .001292$ , and  $e'''$  is so small, as not to admit of being expressed by fewer than six cyphers between the decimal point and first significant figure to the right hand; and, consequently, the ellipse of which it is the excentricity, differs not sensibly from a circle.

HENCE also it follows, that the series of excentricities  $e$ ,  $e'$ ,  $e''$ , &c. continued in the opposite direction, must approach rapidly towards 1, which is their limit; and if we suppose  $e = .5$ , as before, we shall have  $e' = .942809$ ,  $e'' = .999566$ ; as to  $e'''$ , it differs so little from the transverse axis, that the ellipse, of which it denotes the excentricity, may be considered as a straight line. Thus it appears, that the rectification of any ellipse may be reduced to the rectification of two other ellipses, either considerably more excentric than itself, or considerably less excentric.

12. IT is now to be observed, that the two ellipses by which we have expressed the coefficients of the development of the formula  $(a^2 + b^2 - 2ab \cos \phi)^n$ , have the same relation between themselves,

themselves, that any two adjoining ellipses in the series E, E, E', &c. have to each other.

For,  $e$  denoting the excentricity of the ellipse (E), and  $\varepsilon$  that of the ellipse (E'), we have found  $e^2 = \frac{4\varepsilon}{(1+\varepsilon)^2}$ , (Art. 8.) therefore,  $\sqrt{1-e^2} = \frac{1-\varepsilon}{1+\varepsilon}$ , and consequently  $\varepsilon = \frac{1-\sqrt{1-e^2}}{1+\sqrt{1-e^2}}$ , so that we may change the symbols (E) and (E') for E and E', and put  $e'$  for  $\varepsilon$ ; and, since we have the two equations

$$(1+e)E - (2+e)E + e(1+e)E' = 0,$$

$$(1+e')E - (2+e')E' + e'(1+e')E'' = 0,$$

it will immediately follow, that if any of the two ellipses E and E', (by which we have expressed the coefficients A, B, &c.) have such a degree of excentricity, as to be unfavourable to numerical calculation, we can express that ellipse by means of the other one, and a third ellipse, which may be more or less excentric than either of the other two ellipses, just as we please.

13. I SHALL now give, in the form of practical rules, the substance of the preceding investigations for determining the first two coefficients of the development of the formula  $(a^2 + b^2 - 2ab \cos \phi)^n$ , in the case when  $n = -\frac{3}{2}$ ; but from these, and from what has been delivered in Articles 4. and 5., it is easy to determine any number of the coefficients, when  $n$  is the half of any whole number, positive or negative.

To compute A and B in the equation

$$(a^2 + b^2 - 2ab \cos \phi)^{-\frac{3}{2}} = A + B \cos \phi + C \cos 2\phi + \&c.$$

1. FIND E half the perimeter of an ellipse, of which the semi-transverse axis = 1, the semi-conjugate =  $\frac{a-b}{a+b}$ , and therefore, the excentricity =  $\frac{2\sqrt{ab}}{a+b}$  =

2. ALSO

2. ALSO, find  $E'$  half the perimeter of another ellipse, of which the semi-transverse axis = 1, the semi-conjugate axis =  $\frac{\sqrt{a^2 - b^2}}{a}$ , and excentricity =  $\frac{b}{a}$ .

LET  $\pi$  denote 3.14159, &c.

$$\text{Then, } A = \frac{1}{(a+b)(a-b)^2} \cdot \frac{E}{\pi}.$$

$$B = \frac{2a}{b} A - \frac{2}{b(a^2 - b^2)} \cdot \frac{E'}{\pi}.$$

IF either of the ellipses  $E, E'$ , were judged to be of such a form, as that the series for its rectification converged too slowly, then another ellipse, either more or less excentric, might be substituted for it thus:

LET  $e$  and  $c$  denote the excentricity and semi-conjugate axis of the ellipse  $E$ , and  $e'$  and  $c'$  the same things of the ellipse  $E'$ .

3. COMPUTE  $e' = \frac{2\sqrt{e}}{1+e}$ , and  $c' = \frac{1-e}{1+e}$ ; then  $e'$  and  $c'$  will denote the excentricity and semi-conjugate axis of an ellipse more excentric than either  $E'$  or  $E$ .

4. OR, compute  $e'' = \frac{1-e'}{1+e'}$ , and  $c'' = \frac{2\sqrt{e'}}{1+e'}$ ; then  $e''$  and  $c''$  will denote the excentricity and semi-conjugate axis of an ellipse less excentric than either of the same ellipses.

5. COMPUTE  $E'$  half the perimeter of the ellipse, of which the excentricity is  $e'$ .

6. OR, Compute  $E''$  half the perimeter of the ellipse, having its excentricity =  $e''$ .

$$\begin{aligned} \text{Then, } E \left\{ \begin{aligned} &= \frac{1+e}{2+c} E' + \frac{c(1+c)}{2+c} E'' \\ &= \frac{2+c'}{1+e'} E' - \frac{c'(1+c')}{1+e'} E'' \end{aligned} \right. \\ E' \left\{ \begin{aligned} &= \frac{2+c}{c(1+c)} E - \frac{1+e}{c(1+c)} E'' \\ &= \frac{1+e'}{2+c'} E + \frac{c'(1+c')}{2+c'} E'' \end{aligned} \right. \end{aligned}$$

14. THERE are two series, which, so far as I know, are the best adapted to the rectification of the ellipse, of any hitherto published; because, while both are well suited to a moderate degree of excentricity, the one converges with great rapidity when the excentricity is considerable, and the other when it is small.

THE first of these series, which appears to have been originally given by Mr EULER \*, converges by the powers of the semi-conjugate axis, and may be expressed, so as to exhibit the law of the series, as follows:

LET the semi-transverse axis = 1,  
the semi-conjugate axis =  $c$ .

Then,  $\frac{1}{4}$  of the perimeter of the ellipse is equal to

$$1 + (c^2 + \frac{3}{4} \cdot \frac{1}{2} c^4 + \frac{3 \cdot 5}{4 \cdot 6} \cdot \frac{1 \cdot 3}{2 \cdot 4} c^6 + \frac{3 \cdot 5 \cdot 7}{4 \cdot 6 \cdot 8} \cdot \frac{1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6} c^8 + \&c.) \text{ hyp. log. } \frac{2}{\sqrt{c}}$$

$$- c^2 \cdot \frac{1}{4}$$

$$- \frac{3}{4} \cdot \frac{1}{2} c^4 \left( \frac{7}{12} - \frac{1}{2 \cdot 3 \cdot 4} \right)$$

$$- \frac{3 \cdot 5}{4 \cdot 6} \cdot \frac{1 \cdot 3}{2 \cdot 4} c^6 \left( \frac{13}{20} - \frac{1}{2 \cdot 3 \cdot 4} - \frac{1}{4 \cdot 5 \cdot 6} \right)$$

$$- \frac{3 \cdot 5 \cdot 7}{4 \cdot 6 \cdot 8} \cdot \frac{1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6} c^8 \left( \frac{19}{28} - \frac{1}{2 \cdot 3 \cdot 4} - \frac{1}{4 \cdot 5 \cdot 6} - \frac{1}{6 \cdot 7 \cdot 8} \right)$$

$$- \&c.$$

THE other series was given by Mr IVORY, in a very ingenious paper upon the same subject we have been just now considering †. It converges by the difference of the axes, divided by their sum; so that if we suppose the semi-axes, as before, to be denoted by 1 and  $c$ ; and put  $d = \frac{1-c}{1+c}$ , and  $\pi$  for 3.14159, &c.

Half the perimeter

$$= \frac{\pi}{1+d} \left\{ 1 + \frac{1^2}{2^2} d^2 + \frac{1^2 \cdot 1^2}{2^2 \cdot 4^2} d^4 + \frac{1^2 \cdot 1^2 \cdot 3^2}{2^2 \cdot 4^2 \cdot 6^2} d^6 + \frac{1^2 \cdot 1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2 \cdot 8^2} d^8 + \&c. \right\}$$

\* IN a tract entitled "Animadversiones in Rectificationem Ellipsis," which forms part of the second volume of his Opuscula.

† Transactions of the Royal Society of Edinburgh, vol. iv.

I SHALL now give a third series, which, so far as I know, is new; and which, besides possessing the singular advantage of converging rapidly in every case of excentricity that can possibly occur, has other properties that render it peculiarly well fitted to the purpose for which it is wanted in this paper.

LET  $1$  denote the semi-transverse axis, and  $e$  the excentricity of an ellipse, as before.

Find  $e' = \frac{1 - \sqrt{1 - e^2}}{1 + \sqrt{1 - e^2}}$ ,  $e'' = \frac{1 - \sqrt{1 - e'^2}}{1 + \sqrt{1 - e'^2}}$ ,  $e''' = \frac{1 - \sqrt{1 - e''^2}}{2 + \sqrt{1 - e''^2}}$ , &c.

Compute  $P = (1 + e')(1 + e'')(1 + e''')$  &c.

and  $Q = \frac{e}{2} + \frac{ee'}{2 \cdot 2} + \frac{ee'e''}{2 \cdot 2 \cdot 2} + \frac{ee'e''e'''}{2 \cdot 2 \cdot 2 \cdot 2} + \text{\&c.}$

Half the perimeter  $= \pi P(1 - eQ)$ .

IT is evident from the form of this last series, that if  $e$  be considered as the excentricity of  $E$ , one of the ellipses by which we have expressed the coefficients  $A$ ,  $B$ , (Art. 13.) it presently follows, that  $e'$ , the next quantity that occurs in the series, will denote the excentricity of  $E'$ , the other ellipse. Now  $e'$ ,  $e''$ , &c. are the same functions of  $e$ , that  $e''$ ,  $e'''$ , &c. are of  $e'$ . Hence it follows, that if

$$P' = (1 + e'')(1 + e''')(1 + e'''), \text{\&c.}$$

$$Q' = \frac{e'}{2} + \frac{e'e''}{2 \cdot 2} + \frac{e'e''e'''}{2 \cdot 2 \cdot 2} + \text{\&c.}$$

we get,  $E' = \pi P'(1 - e'Q')$ .

BUT in comparing together the values of  $P$  and  $P'$ , also  $Q$  and  $Q'$ , it appears that  $P' = \frac{P}{1 + e'}$ , and  $Q' = \frac{2Q}{e} - 1 = \frac{e(1 + e)^2}{2e'}Q - 1$ , so that the ratios of the ellipses  $E$  and  $E'$  to their common circumscribing circle, (which are what we want in the computation of the coefficients  $A$  and  $B$ ) may be expressed thus:

$$\frac{E}{\pi} = P(1 - eQ).$$

$$\frac{E'}{\pi} = P(1 - \frac{e}{2}(1 + e')Q).$$

15. THE method of determining the coefficients, which is derived from this last series for the rectification of the ellipse, therefore, seems better than such as would be afforded by either of the other two series given in this paper. For, besides considering that there is no quantity required for the computation of  $\frac{E'}{\pi}$ , which was not previously wanted in the computation of  $\frac{E}{\pi}$ , it appears that the logarithms of the quantities  $e'$ ,  $e''$ , &c. also  $1 + e'$ ,  $1 + e''$ , &c. may be all derived from  $e$ , and from each other, with great facility, by means of the common trigonometrical tables, in the following manner.

LET  $\alpha$  be such an arch that  $\sin \alpha = e$ , then  $\cos \alpha = \sqrt{1 - e^2}$ , and consequently  $e' = \frac{1 - \sqrt{1 - e^2}}{1 + \sqrt{1 - e^2}} = \frac{1 - \cos \alpha}{1 + \cos \alpha} = \tan^2 \frac{\alpha}{2}$ , also  $1 + e' = \sec^2 \frac{\alpha}{2}$ . Again, by taking  $\alpha'$  such that  $\sin \alpha' = e'$ , we find, in like manner,  $e'' = \tan^2 \frac{\alpha'}{2}$ , and  $1 + e'' = \sec^2 \frac{\alpha'}{2}$ ; and so on we may go, as far as may be necessary. Hence it follows, that

$$P = \sec^2 \frac{\alpha}{2} \cdot \sec^2 \frac{\alpha'}{2} \cdot \sec^2 \frac{\alpha''}{2}, \text{ \&c.}$$

$$Q = \frac{\sin \alpha}{2} + \frac{\sin \alpha \cdot \sin \alpha'}{2 \cdot 2} + \frac{\sin \alpha \cdot \sin \alpha' \cdot \sin \alpha''}{2 \cdot 2 \cdot 2} + \text{\&c.}$$

and since we are provided with the logarithms of  $\sec^2 \frac{\alpha}{2}$ ,  $\sec^2 \frac{\alpha'}{2}$ , &c. also  $\sin \alpha$ ,  $\sin \alpha'$ , &c. the arithmetical calculation of  $P$  and  $Q$  will be very easy.

I MUST observe that this last method of determining the coefficients  $A$  and  $B$ , in the case of the exponent  $n = -\frac{3}{2}$ , by means of the infinite product  $P = (1 + e')(1 + e'')(1 + e''') \text{ \&c.}$  and the series  $Q = \frac{e}{2} + \frac{ee'}{2 \cdot 2} + \frac{ee'e''}{2 \cdot 2 \cdot 2} + \text{\&c.}$  coincides in the result with the method given by Mr IVORY, for determining the same coefficients, when the exponent is  $-\frac{1}{2}$ , in his very ingenious pa-

per upon that subject, which we have already quoted : his solution, however, is obtained without any reference to the ellipse.

I MIGHT now shew the application of the different methods here investigated, by calculating the values of  $A$  and  $B$  in some particular case, as, for example, in the case of Venus and the Earth ; but as these methods have been sufficiently detailed, so as to be easily understood by any person skilled in arithmetical calculation, I shall conclude this paper, and give, in the form of an Appendix, the investigation of a new formula for the rectification of any elliptic arch whatever ; but from which, in the particular case of that arch becoming a quadrant, we immediately derive the third and last formula mentioned in this paper, for finding the semi-perimeter of an ellipse.

APPEN-



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*APPENDIX, containing the Investigation of a Formula for the Rectification of any Arch of an Ellipse.*

1. **I**T is now generally understood, that by the rectification of a curve line, is meant, not only the method of finding a straight line exactly equal to it, but also the method of expressing it by certain functions of the other lines, whether straight lines or circles, by which the nature of the curve is defined. It is evidently in the latter sense that we must understand the term *rectification*, when applied to the arches of conic sections, seeing that it has hitherto been found impossible, either to exhibit straight lines equal to them, or to express their relation to their co-ordinates, by algebraic equations, consisting of a finite number of terms.

WITH respect to the rectification of the circle and parabola, there seems to remain but little farther to be desired. The determination of any arch of the former of these curves is a problem which so often occurs, and its solution is rendered so easy, by the aid of trigonometrical tables, that formulæ, involving circular arches, are considered as nearly of the same degree of simplicity, as if they involved only algebraic functions of straight lines; and as to the latter curve, it is well known that the formula expressing its length is composed of two parts, the one an algebraic, and the other a logarithmic function of the co-ordinates, so that, by means of a table of logarithms, we can quickly assign the numerical value of any portion of the curve.

2. THE indefinite rectification of the ellipse, or hyperbola, cannot, however, be effected by formulæ of such simplicity as those which express indefinite arches of the two former curves; for the algebraic equations which define their nature, and from which we derive the formulæ for their rectification, are more complex than the equations which define the nature of the circle and parabola. But it is to be observed, that whatever difficulty there may be in the rectification of the ellipse and hyperbola, it is now confined entirely to the ellipse; for by one of the most happy applications that has ever been made of the modern analysis to geometry, it has been discovered, that the rectification of any hyperbolic arch whatever may always be reduced to the rectification of two elliptic arches \*. Thus, it appears, that whatever facility the following series may afford for the rectification of the ellipse, it must also be understood to extend the same to the rectification of the hyperbola, and to every problem, for the solution of which the rectification of either of these curves is necessary.

3. BUT to proceed with the investigation of our formula, let us suppose that  $\phi$  and  $\phi'$  are two arches of a circle, so related to each other, that

$$\frac{\sin 2\phi}{\sqrt{1 + e'^2 + 2e' \cos 2\phi}} = \sin 2\phi',$$

where  $e'$  denotes an invariable quantity.

THEN it is evident, that when  $\phi = 0$ ,  $\phi'$  is also  $= 0$ , and while  $\phi$  increases from 0 to a quadrant or  $\frac{\pi}{2}$ ,  $\phi'$  will also increase from 0 to  $\frac{\pi}{2}$ .

TAKING now the fluxion of our assumed equation, we get,

$$\frac{\dot{\phi} \cos 2\phi}{\sqrt{1 + e'^2 + 2e' \cos 2\phi}} + \frac{e' \dot{\phi} \sin^2 2\phi}{(1 + e'^2 + 2e' \cos 2\phi)^{\frac{3}{2}}} = \dot{\phi}' \cos 2\phi;$$

But

\* THIS discovery was made by Mr LANDEN, who published it first in the Philosophical Transactions for 1775, and afterwards in his Mathematical Memoirs.

But  $\frac{\sin^2 2\phi}{1 + e'^2 + 2e' \cos 2\phi} = \sin^2 2\phi'$ , therefore also

$$\frac{\dot{\phi} \cos 2\phi}{\sqrt{1 + e'^2 + 2e' \cos 2\phi}} + \frac{e' \dot{\phi} \sin^2 2\phi}{\sqrt{1 + e'^2 + 2e' \cos 2\phi}} = \dot{\phi}' \cos 2\phi'.$$

LET us now reduce the terms on the left-hand side of the first fluxionary equation to a common denominator, and we find

$$\frac{\dot{\phi}(1 + e' \cos 2\phi)(e' + \cos 2\phi)}{(1 + e'^2 + 2e' \cos 2\phi)^{\frac{3}{2}}} = \dot{\phi}' \cos 2\phi'.$$

BUT from our assumed equation, we find

$$\frac{e' + \cos 2\phi}{\sqrt{1 + e'^2 + 2e' \cos 2\phi}} = \cos 2\phi'.$$

Hence, and from the last fluxionary equation, we have

$$\frac{\dot{\phi}(1 + e' \cos 2\phi)}{1 + e'^2 + 2e' \cos 2\phi} = \dot{\phi}'.$$

AGAIN, taking the square of both sides of our assumed equation, and multiplying by  $e'^2$ , we have  $\frac{e'^2(1 - \cos^2 2\phi)}{1 + e'^2 + 2e' \cos 2\phi} = e'^2 \sin^2 2\phi'$ , and therefore

$$\frac{1 + e' \cos 2\phi}{\sqrt{1 + e'^2 + 2e' \cos 2\phi}} = \sqrt{1 - e'^2 \sin^2 2\phi'}.$$

LET each side of the last fluxionary equation be now divided by the corresponding side of the last equation; thus we find

$$\frac{\dot{\phi}}{\sqrt{1 + e'^2 + 2e' \cos 2\phi}} = \frac{\dot{\phi}'}{\sqrt{1 - e'^2 \sin^2 2\phi'}};$$

and by comparing this result with the second fluxionary equation, we also find

$$\frac{\dot{\phi} \cos 2\phi}{\sqrt{1 + e'^2 + 2e' \cos 2\phi}} + \frac{e' \dot{\phi}' \sin^2 2\phi'}{\sqrt{1 - e'^2 \sin^2 2\phi'}} = \dot{\phi}' \cos 2\phi'.$$

LET us next assume  $e^2 = \frac{4e'}{(1 + e')^2}$ , then it follows that  $\sqrt{1 - e^2} = \frac{1 - e'}{1 + e'}$ , and  $e' = \frac{1 - \sqrt{1 - e^2}}{1 + \sqrt{1 - e^2}}$ ; if we now substitute  $1 - 2 \sin^2 \phi'$  for  $\cos 2\phi$ , and  $e^2$  for  $\frac{4e'}{(1 + e')^2}$ , in the expression  $1 + e'^2 + 2e' \cos 2\phi$ , it is transformed to  $(1 + e')^2 (1 - e^2 \sin^2 \phi')$ , so that, after due reduction

reduction, our two last fluxionary equations may be otherwise expressed, thus:

$$\frac{\dot{\phi}}{\sqrt{1-e^2 \sin^2 \phi}} = (1+e') \frac{\dot{\phi}'}{\sqrt{1-e'^2 \sin^2 2\phi'}},$$

$$\frac{\dot{\phi} \sin^2 \phi}{\sqrt{1-e^2 \sin^2 \phi}} = \frac{e'(1+e')}{2} \frac{\dot{\phi}' \sin^2 2\phi'}{\sqrt{1-e'^2 \sin^2 2\phi'}} + \frac{1}{2} \frac{\dot{\phi}}{\sqrt{1-e^2 \sin^2 \phi}} - \frac{1+e'}{2} \dot{\phi} \cos 2\phi'.$$

LET us, for the present, denote the fluents of the fluxions which enter these two equations, as follows:

$$P = \int \frac{\dot{\phi}}{\sqrt{1-e^2 \sin^2 \phi}}, \quad P' = \int \frac{\dot{\phi}'}{\sqrt{1-e'^2 \sin^2 2\phi'}},$$

$$N = \int \frac{\dot{\phi} \sin^2 \phi}{\sqrt{1-e^2 \sin^2 \phi}}, \quad N' = \int \frac{\dot{\phi}' \sin^2 2\phi'}{\sqrt{1-e'^2 \sin^2 2\phi'}};$$

and the relation of the fluents will be expressed thus:

$$P = (1+e') P',$$

$$N = \frac{e'(1+e')}{2} N' + \frac{P}{2} - \frac{1+e'}{2.2} \sin 2\phi'.$$

4. Now, remarking that these two equations have been obtained, by assuming

$$e' = \frac{1-\sqrt{1-e^2}}{1+\sqrt{1-e^2}}, \text{ and } \sin 2\phi' = \frac{\sin 2\phi}{\sqrt{1+e'^2+2e' \cos 2\phi}} = \frac{\sin 2\phi}{(1+e')\sqrt{1-e^2 \sin^2 \phi}},$$

let us farther assume two similar equations, thus,

$$e'' = \frac{1-\sqrt{1-e'^2}}{1+\sqrt{1-e'^2}}, \quad \sin 4\phi'' = \frac{\sin 4\phi'}{\sqrt{1+e''^2+2e'' \cos 4\phi'}} = \frac{\sin 4\phi'}{(1+e'')\sqrt{1-e'^2 \sin^2 2\phi'}},$$

where it is evident that when  $\phi' = 0$ ,  $\phi''$  is also  $= 0$ , and that while  $\phi$  or  $\phi'$  increases from  $0$  to  $\frac{\pi}{2}$ ,  $\phi''$  will also increase from  $0$  to  $\frac{\pi}{2}$ .

If we take the fluxions of this second assumed equation, and reason in all respects as in Article 3., we shall arrive at a similar conclusion, that is, putting  $P''$  for  $\int \frac{\dot{\phi}''}{\sqrt{1-e''^2 \sin^2 4\phi''}}$ , and  $N''$  for

$$\int \frac{\dot{\phi}'' \sin^2 4\phi''}{\sqrt{1-e''^2 \sin^2 4\phi''}},$$

$$P' =$$

$$P' = (1 + e'') P',$$

$$N' = \frac{e''(1 + e'')}{2} N'' + \frac{P'}{2} - \frac{1 + e''}{2.4} \sin 4\phi''.$$

IF by means of these two equations, and the two similar equations found in last Article, we exterminate  $P'$  and  $N'$ , we get

$$P = (1 + e') (1 + e'') P'',$$

$$N = e' e'' \frac{(1 + e') (1 + e'')}{2 \cdot 2} N'' + \left\{ \frac{1}{2} + \frac{e'}{2.2} \right\} P - \frac{1 + e'}{2 \cdot 2} \sin 2\phi' - \frac{e' (1 + e') (1 + e'')}{2 \cdot 2 \cdot 2 \cdot 2} \sin 4\phi''.$$

5. LET us now assume two series of equations, such, that all the terms of each series may be similar to one another, and to the equations which we have already assumed, (Art. 3. and 4.) or so that

$$e''' = \frac{1 - \sqrt{1 - e''^2}}{1 + \sqrt{1 - e''^2}}, \sin 8\phi''' = \frac{\sin 8\phi''}{\sqrt{1 + e''^2 + 2e'' \cos 6\phi''}} = \frac{\sin 8\phi''}{(1 + e''') \sqrt{1 - e''^2 \sin^2 4\phi''}},$$

$$e^{iv} = \frac{1 - \sqrt{1 - e'''^2}}{1 + \sqrt{1 - e'''^2}}, \sin 16\phi^{iv} = \frac{\sin 16\phi'''}{\sqrt{1 + e^{iv} + 2e^{iv} \cos 8\phi'''}} = \frac{\sin 16\phi'''}{(1 + e^{iv}) \sqrt{1 - e'''^2 \sin^2 8\phi'''}} \&c.$$

each series being supposed to proceed, according to the same law, as far as we please; then, by due consideration of the quantities  $\sin 8\phi'''$ ,  $\sin 16\phi^{iv}$ , &c. it appears that when  $\phi'' = 0$ , all the remaining arches  $\phi'''$ ,  $\phi^{iv}$ , &c. are also each  $= 0$ , and while  $\phi''$  increases from 0 to  $\frac{\pi}{2}$ , the remaining arches also increase from 0 to  $\frac{\pi}{2}$ ; so that, upon the whole, while  $\phi$ , the first arch, increases from 0 to a quadrant, each of the remaining arches  $\phi'$ ,  $\phi''$ ,  $\phi'''$ , &c. will also increase from 0 to a quadrant.

6. LET us next substitute for the fluents as before

$$P'' = \int \frac{\phi''}{\sqrt{1 - e'^2 \sin^2 \phi''}}, \&c. \quad N''' = \int \frac{\phi''' \sin 8\phi'''}{\sqrt{1 - e''^2 \sin^2 8\phi'''}} \&c.$$

then, following the same analogy as in Articles 3. and 4. we have

$$P' =$$

$$P'' = (1 + e''') P''',$$

$$P''' = (1 + e^{iv}) P^{iv},$$

&c. &c.

$$N'' = \frac{e'''(1 + e''')}{2} N''' + \frac{P''}{2} - \frac{1 + e'''}{2 \cdot 8} \sin 8\phi''',$$

$$N''' = \frac{e^{iv}(1 + e^{iv})}{2} N^{iv} + \frac{P'''}{2} - \frac{1 + e^{iv}}{2 \cdot 16} \sin 16\phi^{iv},$$

&c.

and thus we may proceed as far as we please. If these equations be now combined with the equations similar to them which we have already found (Art. 3. and 4.), and the quantities  $N', P'; N'', P''; N''', P''', \&c.$  be gradually exterminated in the same way as in obtaining the equations of Art 4. we get the following series of equations:

$$P \begin{cases} = (1 + e') P' \\ = (1 + e')(1 + e'') P'' \\ = (1 + e')(1 + e'')(1 + e''') P''', \end{cases}$$

&c.

$$N \begin{cases} = e' \frac{(1 + e')}{2} N' + \frac{1}{2} P - \frac{(1 + e')}{2} \sin 2\phi', \\ = e' e'' \frac{(1 + e')(1 + e'')}{2 \cdot 2} N'' + \left\{ \frac{1}{2} + \frac{e'}{2 \cdot 2} \right\} P - \frac{1 + e'}{2 \cdot 2} \sin 2\phi' - \frac{e'(1 + e')(1 + e'')}{2 \cdot 2 \cdot 2 \cdot 2} \sin 4\phi'', \\ = e' e'' e''' \frac{(1 + e')(1 + e'')(1 + e''')}{2 \cdot 2 \cdot 2} N''' + \left\{ \frac{1}{2} + \frac{e'}{2 \cdot 2} + \frac{e' e''}{2 \cdot 2 \cdot 2} \right\} P - \frac{1 + e'}{2 \cdot 2} \sin 2\phi' \\ - \frac{e'(1 + e')(1 + e'')}{2 \cdot 2 \cdot 2 \cdot 2} \sin 4\phi'' - \frac{e' e''(1 + e')(1 + e'')(1 + e''')}{2 \cdot 2 \cdot 2 \cdot 2 \cdot 2 \cdot 2} \sin 16\phi'''. \end{cases}$$

&c.

If we suppose these values of  $P$  and  $N$  to be continued indefinitely, it is evident that the former will be expressed by the product of an infinite number of invariable factors, and by the limit to which the series of fluents  $P', P'', P''', \&c.$  constantly tends; and the latter by another infinite product, and two infinite series, which involve also  $P$ , and the limits to which  
the

the series of fluents  $N', N'', N''', \&c.$  and the series of arches  $\phi, \phi', \phi'', \&c.$  both constantly tend.

7. Now in order to investigate these limits, I observe, in the first place, that the quantities which constitute the series  $e, e', e'', e''', \&c.$  converge very fast towards 0; for, since  $e' = \frac{1 - \sqrt{1 - e^2}}{1 + \sqrt{1 - e^2}}$ , it is evident that  $e'$  must always be a fraction less than 1, and that  $e'$  must be less than the square of that fraction; indeed, if  $e$  be small,  $e'$  will be nearly  $= \frac{e^2}{4}$ . In the same manner it appears that  $e''$  is between the limits of  $e'^2$  and  $\frac{e'^2}{4}$ , so that  $e''$  is less than  $e'^4$ , but greater than  $\frac{e'^4}{64}$ ; and proceeding in this manner, we find  $e'''$  less than  $e^8$ ;  $e^{iv}$  less than  $e^{16}$ , and so on, the exponents of  $e$  forming the geometrical progression, 2, 4, 8, 16, 32, &c.

Thus it appears, even without taking into account the denominators of the powers of  $e$ , that the rapidity with which the quantities  $e, e', e'', \&c.$  converge towards 0 is indeed very great.

8. I OBSERVE, in the next place, that the terms which constitute the series of arches  $\phi, \phi', \phi'', \phi''', \&c.$  also converge very fast to a certain limit. For, since we have assumed such a relation between the arches that

$$\begin{aligned} \sin 2\phi &= \frac{\sin \phi}{\sqrt{1 + e^2 + 2e \cos \phi}}, \\ \sin 4\phi &= \frac{\sin 2\phi}{\sqrt{1 + e'^2 + 2e' \cos 2\phi}}, \\ \sin 8\phi &= \frac{\sin 4\phi}{\sqrt{1 + e''^2 + 2e'' \cos 4\phi}}, \\ \&c. \end{aligned}$$

it is evident, from what has been said in the last Article, that the denominators of the fractions, which are equal to  $\sin 2\phi, \sin 4\phi, \sin 8\phi, \&c.$  converge quickly towards unity, so that the ratios of  $\sin 2\phi$  to  $\sin 2\phi$ , of  $\sin 4\phi$  to  $\sin 4\phi$ , of  $\sin 8\phi$  to  $\sin 8\phi$ , &c.

approach very fast to the ratio of equality, which is their limit; and the same will also be true of the ratios which every two adjoining arches of the series  $\phi, \phi', \phi'', \phi''', \&c.$  have to each other.

LET us denote the limit, to which these arches continually approach, by  $\theta$ , and it will presently appear, that the same arch  $\theta$  is also the limit of the series of fluents  $\int \frac{\phi}{\sqrt{1-e^2 \sin^2 \phi}}, \int \frac{\phi'}{\sqrt{1-e'^2 \sin^2 2\phi'}}, \int \frac{\phi''}{\sqrt{1-e''^2 \sin^2 4\phi''}}, \&c.$  or of  $P, P', P'', \&c.$  so that we have ultimately

$$P = \theta (1 + e') (1 + e'') (1 + e'''), \&c.$$

9. IN the same way it appears that the series of fluents  $\int \frac{\phi \sin^2 \phi}{\sqrt{1-e^2 \sin^2 \phi}}, \int \frac{\phi' \sin^2 2\phi'}{\sqrt{1-e'^2 \sin^2 2\phi'}}, \int \frac{\phi'' \sin^2 4\phi''}{\sqrt{1-e''^2 \sin^2 4\phi''}}, \&c.$  or  $N, N', N'', \&c.$  continually approaches to  $\int \theta \sin^2 n\theta$ , where  $n$  denotes a number indefinitely great; but in this case  $\int \theta \sin^2 n\theta = \int \theta \left( \frac{1}{2} - \frac{1}{2} \cos 2n\theta \right)$  is equal to  $\frac{\theta}{2}$ , a finite quantity; now if we remark that this quantity, which enters the value of  $N$ , (Art. 6.) is multiplied by the two infinite products  $e'.e''.e'''. \&c.$  and  $\frac{1+e'}{2} \cdot \frac{1+e''}{2} \cdot \frac{1+e'''}{2} \cdot \&c.$  both of which are evidently equal to 0, it appears that the quantity  $e'.e''.e''', \&c. \times \frac{(1+e')(1+e'')(1+e''') \&c.}{2 \cdot 2 \cdot 2} \times \frac{\theta}{2}$  is to be rejected.

10. FINALLY, with regard to the quantity  $N$  itself, or  $\int \frac{\phi \sin^2 \phi}{\sqrt{1-e^2 \sin^2 \phi}}$ , it is evidently equal to  $\frac{1}{2} \left\{ \int \frac{\phi}{\sqrt{1-e^2 \sin^2 \phi}} - \int \phi \sqrt{1-e^2 \sin^2 \phi} \right\}$ ;

now if we put  $1 = AC$ , the semi-transverse axis of an ellipse,  $e =$  the excentricity, and  $\phi = HK$ , any arch of the circumscribing circle, intercepted between the conjugate axis, and  $FG$  an ordinate to the transverse axis, it is well known that



that  $\phi\sqrt{1-e^2\sin^2\phi}$  expresses the fluxion of the elliptic arch FD; let us denote that arch by  $z$ , and we have  $N = \frac{P-z}{e^2}$ ; so that upon the whole we find

$$\frac{P-z}{e^2} = \left\{ + \left( \frac{1}{2} + \frac{e'}{2.2} + \frac{e'^2}{2.2.2} + \frac{e'e''e'''}{2.2.2.2} + \&c. \right) P \right. \\ \left. - \left( \frac{1+e'}{2.2} \sin 2\phi' + \frac{e(1+e')(1+e'')}{2.2.2.2} \sin 4\phi'' + \frac{e'e''(1+e')(1+e'')(1+e''')}{2.2.2.2.2.2} \sin 8\phi''' + \&c. \right) \right.$$

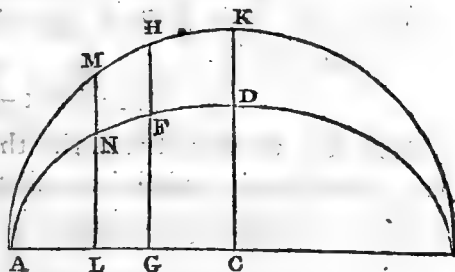
and from this equation the manner of expressing  $z$ , by means of the infinite product  $P$ , and two infinite series, is obvious.

II. I SHALL now collect into one point of view the result of the preceding analysis, in the form of a practical rule for finding the length of any arch of an ellipse.

LET  $I = AC$  the semi-transverse axis,

$e =$  the excentricity,

$z = DF$  any arch of the ellipse, reckoned from the extremity of the conjugate axis.



$\phi = HK$ , an arch of the circumscribing circle, intercepted between the conjugate axis and  $FG$ , an ordinate to the transverse.

COMPUTE the quantities  $e', e'', e''', \&c.$   $\sin 2\phi', \sin 4\phi'', \sin 8\phi''', \&c.$  from these two series of equations,

$$\begin{aligned} e' &= \frac{1 - \sqrt{1-e^2}}{1 + \sqrt{1-e^2}}, & \sin 2\phi' &= \frac{\sin 2\phi}{(1+e')\sqrt{1-e^2\sin^2\phi}}, \\ e'' &= \frac{1 - \sqrt{1-e'^2}}{1 + \sqrt{1-e'^2}}, & \sin 4\phi'' &= \frac{\sin 4\phi'}{(1+e'')\sqrt{1-e'^2\sin^2\phi'}}, \\ e''' &= \frac{1 - \sqrt{1-e''^2}}{1 + \sqrt{1-e''^2}}, & \sin 8\phi''' &= \frac{\sin 8\phi''}{(1+e''')\sqrt{1-e''^2\sin^2\phi''}}, \\ e^{iv} &= \frac{1 - \sqrt{1-e'''^2}}{1 + \sqrt{1-e'''^2}}, & \sin 16\phi^{iv} &= \frac{\sin 16\phi'''}{(1+e^{iv})\sqrt{1-e'''^2\sin^2\phi'''}}. \end{aligned}$$

&c.

&c.

N n z

THE

THE quantities  $e', e'', e'''$ , &c. approach very fast to 0, and the arches  $\phi', \phi'', \phi'''$ , &c. approach to a certain limit, which let us denote by  $\theta$ .

COMPUTE also these three quantities,

$$P = (1 + e')(1 + e'')(1 + e''') \&c.$$

$$Q = \frac{e}{2} + \frac{ee'}{2 \cdot 2} + \frac{ee'e''}{2 \cdot 2 \cdot 2} + \frac{ee'e''e'''}{2 \cdot 2 \cdot 2 \cdot 2} + \&c.$$

$$R = \frac{e(1+e)}{2 \cdot 2} \sin 2\phi' + \frac{ee'(1+e)(1+e'')}{2 \cdot 2 \cdot 2 \cdot 2} \sin 4\phi'' + \frac{ee'e''(1+e)(1+e'')(1+e''')}{2 \cdot 2 \cdot 2 \cdot 2 \cdot 2 \cdot 2} \sin 8\phi''' + \&c.$$

The elliptic arch  $z = \theta P(1 - eQ) + eR$ .

WHEN  $\phi$  becomes a quadrant, the sines of  $2\phi', 4\phi'', 8\phi'''$ , &c. are evidently each 0, so that, putting  $\pi = 3.14159$ , &c. we have in this case,  $\theta = \frac{\pi}{2}$ , and, putting  $E$  to denote the whole elliptic quadrant, we have

$$E = \frac{\pi}{2} P(1 - eQ).$$

Now it is worthy of remark, that the expression  $P(1 - eQ)$  is common to the indefinite arch  $z$  and the whole quadrant  $E$ ; hence it follows that the indefinite arch may be also expressed thus,

$$z = \frac{2\theta}{\pi} E + eR.$$

12. FROM this last formula we may derive many of those curious relations which are known to subsist between certain assignable elliptic arches, as also between these arches and the whole elliptic quadrant \*. For example, we may hence deduce that very remarkable property of the ellipse, which is commonly known by the name of FAGNANI'S theorem, namely, that an elliptic arch, reckoned from the extremity of either axis being supposed given, another arch, reckoned from the extremity of the other axis, may be found, by a geometrical construction,

$$\sqrt{1 - e^2} = \sqrt{1 - e'^2} \quad \text{such}$$

\* THE properties of the ellipse here alluded to have been explained by EULER, and some of them have also been observed by LANDEN.

such, that the difference of these arches shall be equal to a certain assignable straight line.

FOR, let  $(z) = DN$ , another elliptic arch, reckoned from the extremity of the transverse, and  $(\phi) = KM$ , the corresponding arch of the circumscribing circle; then, if we find  $(\phi')$ ,  $(\phi'')$ ,  $(\phi''')$ , &c. a series of arches similar to the series  $\phi'$ ,  $\phi''$ ,  $\phi'''$ , &c. (Art. 11.) or such that

$$\sin 2(\phi') = \frac{\sin 2(\phi)}{(1+e')\sqrt{1-e'^2 \sin^2(\phi)}},$$

$$\sin 4(\phi'') = \frac{\sin 4(\phi')}{(1+e'')\sqrt{1-e''^2 \sin^2 2(\phi')}},$$

$$\sin 8(\phi''') = \frac{\sin 8(\phi'')}{(1+e''')\sqrt{1-e'''^2 \sin^2 4(\phi'')}},$$

&c.

and put  $(\theta)$  to denote the limit of the arches  $(\phi)$ ,  $(\phi')$ ,  $(\phi'')$ , &c. also,

$$(R) = \frac{(1+e')}{2 \cdot 2} \sin 2(\phi') + \frac{e'e'(1+e')(1+e'')}{2 \cdot 2 \cdot 2 \cdot 2} \sin 4(\phi'') + \frac{e'e'e''(1+e')(1+e'')(1+e''')}{2 \cdot 2 \cdot 2 \cdot 2 \cdot 2 \cdot 2} \sin 8(\phi''') + \&c.$$

it follows, that  $(z) = \frac{2(\theta)}{\pi} E + e(R)$ .

Now if we suppose the arches  $\phi$  and  $(\phi)$  to be such that  $\phi' + (\phi') = \frac{\pi}{2}$ , then it follows that  $\sin 2\phi' = \sin 2(\phi')$ , and  $\sin 4\phi' = -\sin 4(\phi')$ , and since

$$\sin 4\phi'' = \frac{\sin 4\phi'}{(1+e'')\sqrt{1-e''^2 \sin^2 2\phi'^2}},$$

$$\text{also, } \sin (4\phi'') = \frac{\sin 4(\phi'')}{(1+e'')\sqrt{1-e''^2 \sin^2 2(\phi'')}},$$

we have  $\sin 4\phi'' = -\sin 4(\phi'')$ . Now when  $\phi' = 0$ , then  $(\phi') = \frac{\pi}{2}$ ; but when  $\phi' = 0$ , it appears (Art. 5.) that  $\phi'' = 0$ , and also, that when  $(\phi') = \frac{\pi}{2}$ , then  $(\phi'') = \frac{\pi}{2}$ ; therefore, when  $4\phi'' = 0$ ,  $4(\phi'') = 2\pi$ . Again, while  $\phi'$  increases from 0 to  $\frac{\pi}{4}$ ,  $(\phi')$  will decrease from  $\frac{\pi}{2}$  to  $\frac{\pi}{4}$ ; hence, and from the two last equations, it

follows,

follows, that in the same circumstances  $4\phi''$  will increase from 0 to  $\pi$ , and  $4(\phi'')$  will decrease from  $2\pi$  to  $\pi$ ; and because that while  $\phi'$  increases from  $\frac{\pi}{4}$  to  $\frac{\pi}{2}$ ,  $(\phi')$  diminishes from  $\frac{\pi}{4}$  to 0, it will also follow, that while  $4\phi''$  increases from  $\pi$  to  $2\pi$ ,  $4(\phi'')$  will decrease from  $\pi$  to 0; and since during these changes of the value of the arches  $4\phi''$ ,  $4(\phi'')$ , we have always  $\sin 4\phi'' = -\sin 4(\phi'')$ , it is evident that  $4\phi'' + 4(\phi'') = 2\pi$ , or  $\phi'' + (\phi'') = \frac{\pi}{2}$ .

AGAIN, from the two equations,  $\sin 8\phi''' = \frac{\sin 8\phi''}{(1+e'')\sqrt{1-e''^2\sin^2 4\phi''}}$ ,  $\sin 8(\phi''') = \frac{\sin 8(\phi'')}{(1+e'')\sqrt{1-e''^2\sin^2 4(\phi'')}}$ , and from the consideration that  $\phi'' + (\phi'') = \frac{\pi}{2}$ , we find  $\sin 8\phi''' = -\sin 8(\phi''')$ ; and hence, by reasoning as before, we also find  $\phi''' + (\phi''') = \frac{\pi}{2}$ ; so that, upon the whole, we obtain the two following series of equations:

$$\sin 2\phi' = +\sin 2(\phi') \quad \phi' + (\phi') = \frac{\pi}{2},$$

$$\sin 4\phi'' = -\sin 4(\phi'') \quad \phi'' + (\phi'') = \frac{\pi}{2},$$

$$\sin 8\phi''' = -\sin 8(\phi''') \quad \phi''' + (\phi''') = \frac{\pi}{2},$$

&c.

&c.

and therefore ultimately

$$\theta + (\theta) = \frac{\pi}{2}.$$

Now we have the elliptical arch DF, or  $z = \frac{2\theta}{\pi} E + eR$ ;

also the arch DN, or  $(z) = \frac{2(\theta)}{\pi} E + e(R)$ .

$$\text{Hence } DN + DF = z + (z) = \frac{2\{\theta + (\theta)\}}{\pi} E + e\{R + (R)\};$$

but, from the two preceding series of equations, it is evident that  $\frac{2\{\theta + (\theta)\}}{\pi} = 1$ , and  $e\{R + (R)\} = \frac{e^2(1+e')}{2} \sin 2\phi'$ ; therefore

$$\begin{aligned} DN + DF &= E + \frac{e^2(1+e')}{2} \sin 2\phi' \text{ and } AN - DF = \frac{e^2(1+e')}{2} \sin 2\phi' \\ &= \frac{e^2 \sin 2\phi}{2\sqrt{1-e^2 \sin^2 \phi}} = \frac{e^2 \sin \phi \cdot \cos \phi}{\sqrt{1-e^2 \sin^2 \phi}}. \end{aligned}$$

THUS

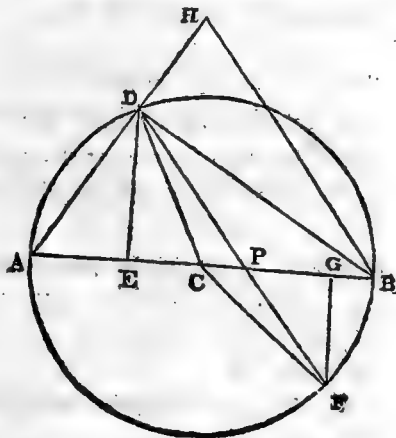
THUS we have two elliptic arches, reckoned from the extremities of the axes, the difference of which is equal to a certain assignable straight line.

13. HAVING found this very curious relation between the elliptic arches AN and FD, let us next investigate the relation between the corresponding circular arches AM, HK, by means of the equation  $\phi' + (\phi') = \frac{\pi}{2}$ .

LET  $\psi = AM$  the arch of the circumscribing circle, between the extremity of the transverse axis, and the ordinate LNM, then  $\cos \psi = \sin(\phi)$ , and  $\sin 2\psi = \sin 2(\phi)$ ; and since  $\sin 2\phi' = \sin 2(\phi')$ , also  $\sin 2\phi' = \frac{\sin 2\phi}{(1+e')\sqrt{1-e'^2\sin^2\phi}}$ , and  $\sin 2(\phi') = \frac{\sin 2(\phi)}{(1+e')\sqrt{1-e'^2\sin^2(\phi)}}$ , it follows, that to determine the relation between  $\phi$  and  $\psi$ , we have

$$\frac{\sin 2\phi}{(1+e')\sqrt{1-e'^2\sin^2\phi}} = \frac{\sin 2\psi}{(1+e')\sqrt{1-e'^2\cos^2\psi}}, \text{ or, } \frac{\sin 2\phi}{\sqrt{1+e'^2+2e'\cos 2\phi}} = \frac{\sin 2\psi}{\sqrt{1+e'^2-2e'\cos 2\psi}}.$$

LET AB be the diameter of a circle, of which AC the radius  $= 1$ ; let the arch AD  $= 2\phi$ , and BF (taken in the opposite femi-circle)  $= 2\psi$ , take CP  $= e'$ , join PD, PF, CD, CF, and draw DE and FG perpendicular to the diameter; then DE  $= \sin 2\phi$ , and EC  $= \cos 2\phi$ ; also FG  $= \sin 2\psi$ , and GC  $= \cos 2\psi$ .



FROM the elements of geometry we have

$$\begin{aligned} PD^2 &\begin{cases} = DC^2 + CP^2 + 2PC \cdot CE, \\ = 1 + e'^2 + 2e' \cos 2\phi; \end{cases} \\ \text{also } PF^2 &\begin{cases} = FC^2 + CP^2 - 2PC \cdot CG, \\ = 1 + e'^2 - 2e' \cos 2\psi. \end{cases} \end{aligned}$$

therefore,

therefore,  $\frac{\sin 2\phi}{\sqrt{1+e'^2+2e'\cos 2\phi}} = \frac{DE}{PD}$ , and  $\frac{\sin 2\psi}{\sqrt{1+e'^2-2e'\cos 2\psi}} = \frac{FG}{PF}$ ,

hence it appears that  $\frac{DE}{PD} = \frac{FG}{PF}$ , so that the triangles PED, PGF, are similar; and, as it is obvious, from what has been already shewn, that the arches AD, BF, are either both less or both greater than quadrants, it follows that PD and PF lie in the same straight line. Join AD and BD, and draw BH parallel to DF, meeting AD in H, then  $AP:PB::AD:DH::\tan ABD:\tan DBH$  or  $\tan BDF$ ; that is,  $1+e':1-e'::\tan \phi:\tan \psi$ ; therefore,  $\tan \psi = \frac{1-e'}{1+e'} \tan \phi$ . But if we denote the semi-conjugate

axis of the ellipse by  $c$ , we have  $e' = \frac{1-c}{1+c}$  (Art 11.); and therefore  $c = \frac{1-e'}{1+e'}$ ; so that we at last derive from our formula, the following very remarkable theorem, first observed by Count Fagnani.

LET AD be a quadrant of an ellipse, of which the semi-transverse axis = 1\*, and AK a quadrant of the circumscribing circle; let  $c$  = the semi-conjugate axis, and  $e'$  = the excentricity; let  $\psi$  = AM, an arch of the circle, reckoned from the extremity of the transverse, and  $\phi$  = HK, an arch of the circle, reckoned from its intersection with the conjugate axis; draw ML and HG perpendicular to the transverse axis, meeting the ellipse in N and F; then, if the arches  $\psi$  and  $\phi$  be such, that  $\tan \psi = c \tan \phi$ , the difference of the elliptic arches AN, FD, is equal to

$$\frac{e' \sin \phi \cos \phi}{\sqrt{1-e'^2 \sin^2 \phi}}.$$

14. It would not be difficult to shew, that by means of our formula, other theorems similar to that of Fagnani, might be investigated; but as these, I believe, have been pointed out by other writers, and as this paper has extended to a greater length than was at first intended, I pass them over for the present, and

proceed,

\*See Fig. page 279.

proceed in the next place to remark, that although in general the quantity  $R$  be expressed by an infinite series, yet there are certain values of the arch  $\phi$ , such, that the series will terminate; and when this happens, the limiting arch  $\theta$  can be found exactly; so that in each of these cases, we obtain a finite equation expressing a relation between the whole elliptic quadrant, and a particular arch of it.

FOR we have already observed (Art. 5.), that while  $\phi$  increases from 0 to a quadrant, or  $\frac{\pi}{2}$ ; each of the arches  $\phi'$ ,  $\phi''$ ,  $\phi'''$ , &c. increases from 0 to a quadrant; so that when  $\phi = \frac{\pi}{2}$ , then  $\sin 2\phi'$ ,  $\sin 4\phi''$ ,  $\sin 8\phi'''$ , &c. are each = 0, and  $\theta = \frac{\pi}{2}$ ; now in the very same way it appears, that while  $2\phi'$  increases from 0 to  $\frac{\pi}{2}$ , each of the arches  $2\phi''$ ,  $2\phi'''$ , &c. increases from 0 to  $\frac{\pi}{2}$ ; so that, in this case,  $\sin 2\phi' = 1$ , and  $\sin 4\phi''$ ,  $\sin 8\phi'''$ , and all that follow are each = 0; hence also the limiting arch  $\theta = \frac{\pi}{4}$ ; and to determine the value of  $\phi$ , we have this equation  $\frac{\sin 2\phi}{\sqrt{1+e'^2+2e'\cos 4\phi}} = \sin 2\phi' = 1$ , from which we find  $\cos 2\phi = -e'$ .

15. REASONING in this way, we find that the series  $R$  will consist of a finite number of terms, and the limit  $\theta$  be exactly assignable in innumerable other cases, namely, when any one of the following series of equations takes place:

$$\cos 2\phi = -e', \text{ the limit } \theta \text{ being then } = \frac{\pi}{4},$$

$$\cos 4\phi' = -e'', \quad \frac{\pi}{8},$$

$$\cos 8\phi'' = -e''', \quad \frac{\pi}{16},$$

$$\&c. \quad \&c.$$

AND since, by means of the series of equations given in Article 11, we can determine  $\phi$ , so that each of the above equations

tions shall successively have place, it appears, that we can assign an indefinite number of elliptic arches, reckoned from the extremity of the conjugate axis, such, that their relation to the whole elliptic quadrant shall be expressible by a finite equation. But we have seen (Art. 13.) that, corresponding to each of these arches, there is another, reckoned from the extremity of the transverse axis, such, that their difference may be expressed by a certain straight line; therefore, it appears that we can assign innumerable arches, reckoned from the extremity of either axis, possessing each the remarkable property of having their relation to the whole elliptic quadrant expressible by a finite algebraic equation\*.

16. For example, let us suppose  $\cos 2\phi = -e'$ , and therefore  $\sin^2 \phi = \frac{1+e'}{2} = \frac{1}{1+e}$ , then, since  $\sin 2\phi' = 1$ , and  $\sin 4\phi''$ ,  $\sin 8\phi'''$ , &c. are each  $= 0$ , we have  $eR = \frac{e^2(1+e')}{2-2} = \frac{e'}{1+e} = \frac{1-e}{2}$ ; now we have also  $\theta = \frac{\pi}{4}$ , therefore  $z = \frac{E}{2} + \frac{1-e}{2}$ ; hence we derive the following very curious proposition:

LET the semi-axes of an ellipse be  $1$  and  $e$ ; take a straight line in the transverse axis, from the centre towards either vertex,  $= \frac{1}{\sqrt{1+e}}$ , and at the extremity of that line, between the centre and vertex, draw an ordinate to the transverse

\* HENCE, by the way, it appears, that instead of the semi-perimeters of two ellipses which we have used in the preceding paper, for expressing the coefficients of the development of  $(a^2 + b^2 - 2ab \cos \phi)^n$ , we may substitute any two of an indefinite number of elliptic arches, and certain algebraic functions of the axes of these ellipses; therefore, the different infinite series, which may be used to express the coefficients  $A$  and  $B$ , are really innumerable.



verse axis, meeting the curve, and dividing the elliptic quadrant into two parts, the greatest of which is that next to the vertex of the conjugate axis; then, if  $E$  denote the whole elliptic quadrant, the greatest elliptic arch shall be equal to  $\frac{E+1-c}{2}$ , and the least  $= \frac{E-1+c}{2}$ ; also the difference of the arches  $= 1-c$  = difference of the semi-axes; and the same conclusion is readily drawn from the theorem given in Art. 13. by assuming  $\phi + \psi = \frac{\pi}{2}$ .

17. ANOTHER observation I have to make upon our Formula is, that we may hence deduce that relation between indefinite arches of three certain ellipses, which is mentioned at Article 10. of the preceding paper, and which was first observed by Mr LEGENDRE; but as the only use we have made of that property was in the case of those arches being quadrants, or semi-perimeters, I shall, for the sake of brevity, treat only of that particular case.

SUPPOSING, therefore, the quantities  $e', e'', e'''$ , &c. to be related to each other as already expressed in Art. 11. we have found, that if

$P = (1+e')(1+e'')(1+e''')$ , &c. and  $Q = \frac{e}{2} + \frac{ee'}{2.2} + \frac{ee'e''}{2.2.2} + \&c.$  and  $E$  denote the quadrant of an ellipse, of which the excentricity is  $e$ , then  $E = \frac{\pi}{2} P (1 - eQ)$ .

IN like manner, if  $E'$  denote the quadrant of another ellipse, of which the excentricity is  $e'$ , and we put

$P' = (1+e'')(1+e''')(1+e''')$ , &c. also  $Q' = \frac{e'}{2} + \frac{e'e''}{2.2} + \frac{e'e'e'''}{2.2.2} + \&c.$

for the very same reason  $E' = \frac{\pi}{2} P' (1 - e'Q')$ .

Q. 6. 2 — — — — — But

BUT by comparing together the values of  $P$  and  $P'$ , also of  $Q$  and  $Q'$ , it appears that  $P = (1 + e')P'$ , and  $Q = \frac{e'}{2}(1 + Q')$ ; so that, by substituting these values of  $P$  and  $Q$  in the formula  $E = \frac{\pi}{2}P(1 - eQ)$ , and remembering that  $e^2 = \frac{4e'}{(1+e')^2}$ , we also get  $(1 + e')E = \frac{\pi}{2}P'(1 + e'^2 - 2e'Q')$ ; and if, by means of this equation, and the equation,  $E' = \frac{\pi}{2}P'(1 - e'Q')$ , we exterminate  $Q'$ , it will be found that

$$2E' - (1 + e')E = (1 - e'^2)\frac{\pi}{2}P'.$$

LET us now put  $E''$  to denote the quadrant of a third ellipse of which the excentricity is  $e''$ ; then, if we also assume

$$P'' = (1 + e''') (1 + e''') (1 + e'''), \text{ \&c., and } Q'' = \frac{e''}{2} + \frac{e''e'''}{2.2} + \frac{e''e''''e'''}{2.2.2} + \text{\&c.}$$

and consider that the second and third ellipses are related to each other exactly in the same manner as the first and second, we get an equation similar to the last, namely,

$$2E'' - (1 + e'')E' = (1 - e''^2)\frac{\pi}{2}P'';$$

now since  $P'' = \frac{P'}{1 + e''}$ , we can readily, by means of these two equations, exterminate  $P'$ ; accordingly we find

$$(1 + e')(1 - e'')E - \left\{ 2(1 - e'') + (1 - e'^2)(1 + e'') \right\} E' + 2(1 - e'^2)E'' = 0.$$

THUS we have got an equation expressing a very remarkable connection between three elliptic quadrants, the excentricities being  $e$ ,  $e'$ , and  $e''$ . But this equation may be rendered more simple in its form; for if we put  $c'$  for the semi-conjugate axis of the second ellipse, we have  $e'' = \frac{1 - e'}{1 + e'}$ , (Art. 11.) and therefore,

$1 + e'' = \frac{1 - e'}{c'}$ ; now  $1 - e'^2 = c'^2$ , therefore  $(1 - e'^2)(1 + e'') = (1 - e'')c'$ ; hence also we have  $2(1 - e'^2) = (1 - e'')c'(1 + c')$ , so that by substituting for  $(1 - e'^2)(1 + e'')$  and  $2(1 - e'^2)$  their values, and

and dividing the whole equation by  $(1 - e'')$ , we at last get

$$(1 + e')E - (2 + e')E' + e'(1 + e')E'' = 0,$$

the very equation which we proposed to investigate.

18. THE last remark I shall make at present, upon the formula given in this appendix for the rectification of the ellipse, relates to the quickness of its convergency; and this, it appears, will in every case be very great. For we have seen (Art. 7.) that  $e'$  is less than the square of the excentricity,  $e''$  less than its fourth power,  $e'''$  less than its eighth power, and so on, proceeding according to the geometrical progression 2, 4, 8, 16, 32, &c.; therefore, if only the three quantities  $e'$ ,  $e''$ ,  $e'''$ , be computed, together with the corresponding arches  $\phi'$ ,  $\phi''$ ,  $\phi'''$ , and substituted in the formula  $z = \theta P(1 - eQ) + eR$ , rejecting all the factors of  $P$ , and all the terms of the series  $Q$  and  $R$ , which involve the quantity  $e^{iv}$ , &c. the part of the formula thus rejected will be multiplied by the 32d power of the excentricity; but if the terms involving  $e^{iv}$  be taken in, the terms rejected will be multiplied by the 64th power, and so on; thus it appears, that the method here given for the rectification of the ellipse is applicable to every case of excentricity, and to every length of an arch that can possibly occur in calculation. Now these are advantages which no other individual series, with which I am acquainted, possesses; for the common series, which converges by the powers of the excentricity, is of no use when the excentricity is great, and it is then necessary to have recourse to a series converging by the powers of the lesser axis. But the series of this form, which is commonly known, converges only for a certain portion of the elliptic arch, and beyond that portion it diverges; so that if it be required to rectify an elliptic arch beyond a certain limit, it is necessary to find the length of the whole quadrant, and thence, by the aid of FAGNANI's theorem, to find the length of the arch which was proposed.

19. I SHALL

19. I SHALL conclude this appendix with an example of the application of our formula; and in the determination of the logarithms of  $e'$ ,  $e''$ , &c.  $1 + e'$ ,  $1 + e''$ , &c. by means of the trigonometrical tables, I shall avail myself of the remarks which have been made for that purpose at Art. 15. of the preceding paper. As to the quantities  $\sin 2\phi$ ,  $\sin 4\phi$ , &c. it is easy to see, from their relation to each other, that their logarithms may be readily determined by means of the same tables.

EXAMPLE. It is required to determine the length of an elliptic arch, when  $e$ , the excentricity,  $= \sqrt{\frac{1}{2}} = .7071068$ , and  $\phi$ , the arch of the circumscribing circle  $= 70^\circ$ , the semi-transverse being supposed  $= 1$ .

I. CALCULATION of the logarithms of  $e'$ ,  $1 + e'$ , &c.

		Logarithms.
$e =$	$\sin \alpha = \sin 45^\circ, 0', 0''$	$\bar{1}.8494850$
$e' = \tan \frac{\alpha}{2} = \sin \alpha' = \sin 9^\circ, 52', 45''.41$		$\bar{1}.2344486$
$1 + e' = \sec \frac{\alpha}{2}$		$0.0687694$
$e'' = \tan \frac{\alpha'}{2} = \sin \alpha'' = \sin 0^\circ, 25', 40''.7$		$\bar{3}.8733008$
$1 + e'' = \sec \frac{\alpha'}{2}$	- - - - -	$0.0032320$
$e''' = \tan \frac{\alpha''}{2}$	- - - - -	$\bar{5}.1444734$
$1 + e''' = \sec \frac{\alpha''}{2}$	- - - - -	$0.0000060$

BECAUSE  $e^{iv}$  is nearly equal to one-fourth of the square of  $e'''$ , (Art. 7.) it is evident that  $e^{iv}$ ,  $1 + e^{iv}$ , &c. may be all neglected.

2. CALCULATION

2. CALCULATION of the logarithms of  $\sin 2\phi'$ ,  $\sin 4\phi'$ , &c.

	Logarithms.
$\sin \phi$	$\overline{1.9729858}$
$e$	$\overline{1.8494850}$
$e \sin \phi = \sin \beta$	$\overline{1.8224708}$
$\sqrt{1 - e^2 \sin^2 \phi} = \cos \beta$	$\overline{1.8735073}$
$1 + e'$	$\overline{0.0687694}$
$(1 + e') \sqrt{1 - e^2 \sin^2 \phi}$	$\overline{1.9422767}$
$\sin 2\phi$	$\overline{1.8080675}$
$\sin 2\phi' = \sin 132^\circ, 45', 49''.3$	$\overline{1.8657908}$
$e'$	$\overline{1.2344486}$
$e' \sin 2\phi' = \sin \beta'$	$\overline{1.1002394}$
$\sqrt{1 - e'^2 \sin^2 2\phi'} = \cos \beta'$	$\overline{1.9965270}$
$1 + e''$	$\overline{0.0032320}$
$(1 + e'') \sqrt{1 - e'^2 \sin^2 2\phi'}$	$\overline{1.9997590}$
$\sin 4\phi'$	$\overline{1.9986754}$
$\sin 4\phi'' = \sin 265^\circ, 57'.15''$	$\overline{1.9989164}$
$e''$	$\overline{3.8733008}$
$e'' \sin 4\phi'' = \sin \beta''$	$\overline{3.8722172}$
$\sqrt{1 - e''^2 \sin^2 4\phi''} = \cos \beta''$	$\overline{1.9999879}$
$1 + e'''$	$\overline{0.0000060}$
$(1 + e''') \sqrt{1 - e''^2 \sin^2 4\phi''}$	$\overline{1.9999939}$
$\sin 8\phi''$	$\overline{1.1484707}$
$\sin 8\phi''' = \sin 531^\circ, 54'.30''$	$\overline{1.1484768}$
	the

the next term,  $\sin 16\phi''$ , being multiplied by  $e''$ , a very small fraction, is on that account to be neglected, as well as all the terms which follow it, for similar reasons.

To determine  $\theta$ , we have  $\phi' = 66^\circ. 22'. 54''$ .

$$\phi'' = 66^\circ. 29'. 18''. 75$$

$$\phi''' = 66^\circ. 29'. 18''. 75$$

&c.

Hence it appears that  $\theta = 66^\circ. 29'. 18''. 75 = 1.1604440$ .

### 3. COMPUTATION OF $\theta P(1 - eQ)$ .

	Logarithms.
$\frac{e^2}{2} = .2500000$	$\bar{1}.3979400$
$\frac{e^2 e'}{2.2} = .0214466$	$\bar{2}.3313586$
$\frac{e^2 e' e''}{2.2.2} = .0000801$	$\bar{5}.9036294$
$\frac{e^2 e' e'' e'''}{2.2.2.2} = .0000000$	$\bar{10}.7470728$

$$eQ = .2715267$$

I.

$1 - eQ = .7284733$	$\bar{1}.8624137$
$1 + e' \quad - \quad -$	$0.0687694$
$1 + e'' \quad - \quad -$	$0.0032320$
$1 + e''' \quad - \quad -$	$0.0000060$
$\theta \quad - \quad -$	$0.0646242$

$$\theta P(1 - eQ) = .9978042 \quad \bar{1}.9990453$$

### 4. COMPUTATION

4. COMPUTATION of  $eR$ , and thence of  $z$  the arch required.

	Logarithms.
$\frac{e^2(1+e)}{2 \cdot 2} \sin 2\phi' = +.1075153$	$\bar{1}.0314702$
$\frac{e^2 e'(1+e')(1+e'')}{2 \cdot 2 \cdot 2 \cdot 2} \sin 4\phi'' = -.0063127$	$\bar{3}.8002164$
$\frac{e^2 e' e''(1+e')(1+e'')(1+e''')}{2 \cdot 2 \cdot 2 \cdot 2 \cdot 2 \cdot 2} \sin 8\phi''' = +.0000017$	$\bar{6}.2210236$
$eR = .1012043$	
$\theta P(1 - eQ) = .9978042$	
The elliptic arch $z = 1.0990085$	

It appears from the preceding calculation, that the length of the elliptic arc would in this case have been found with nearly the same degree of accuracy, if instead of computing the three quantities  $e'$ ,  $e''$ ,  $e'''$ , also  $\sin 2\phi'$ ,  $\sin 4\phi''$ , and  $\sin 8\phi'''$ , we had only computed two of each series, viz.  $e'$  and  $e''$  of the one, and  $\sin 2\phi'$ ,  $\sin 4\phi''$ , of the other series; or, supposing all the three quantities of each series to have been computed to a sufficient number of places of figures, it is evident that we might thence have found the length of the arch to several more places of figures than we have here made use of.

1. The first step in the process of the investigation is the identification of the problem. This is done by the investigator who is responsible for the study. The next step is to collect data. This is done by the investigator who is responsible for the study. The next step is to analyze the data. This is done by the investigator who is responsible for the study. The next step is to interpret the results. This is done by the investigator who is responsible for the study. The next step is to draw conclusions. This is done by the investigator who is responsible for the study. The next step is to report the findings. This is done by the investigator who is responsible for the study. The next step is to discuss the implications. This is done by the investigator who is responsible for the study. The next step is to write the report. This is done by the investigator who is responsible for the study. The next step is to submit the report. This is done by the investigator who is responsible for the study. The next step is to publish the report. This is done by the investigator who is responsible for the study. The next step is to disseminate the findings. This is done by the investigator who is responsible for the study. The next step is to evaluate the process. This is done by the investigator who is responsible for the study. The next step is to improve the process. This is done by the investigator who is responsible for the study. The next step is to repeat the process. This is done by the investigator who is responsible for the study. The next step is to conclude the process. This is done by the investigator who is responsible for the study. The next step is to end the process. This is done by the investigator who is responsible for the study.

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1. The first part of the document is a letter from the President of the United States to the Congress, dated January 8, 1879. It contains a report on the state of the Union and the progress of the government during the year ending December 31, 1878.

[illegible]

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XII. CHEMICAL ANALYSIS of an UNCOMMON SPECIES of ZEOLITE. By ROBERT KENNEDY, M. D., F. R. S., F. A. S., and Fellow of the Royal College of Physicians, Edinburgh.

[Read Jan. 11. 1802.]

THE zeolite subjected to the following experiments, possesses some of the distinguishing properties common to other stones of the same class, but differs in certain respects from any variety with which I am acquainted. I found it, more than three years ago, in the basaltic rock on which the Castle of Edinburgh is built; and it was inclosed within a mass of prehnite.

THE colour of this zeolite is in some parts nearly white, in others greyish white. It is composed entirely of straight fibres, arranged in bundles or masses of different sizes, all the fibres of each mass converging towards a common point. The whole specimen is an aggregate of these masses, the bases of which are in contact with the prehnite, and are impressed with the form of its surface, which is rounded, or botryoidal. The cross fracture presents the irregular and ragged ends of the broken fibres, termed the *hackly* fracture by Mr KIRWAN.

ALTHOUGH the shape and arrangement of the fibres appear plainly to be the effects of crystallization, yet I have not been able to trace a perfectly regular, determinate form in any of them. However, when the stone is broken, some of them can be readily separated longitudinally, in a pretty entire state, and seem in most

instances to be four-sided and rectangular. Their length is from half an inch to two inches; but their thickness does not exceed one-fortieth or one-fiftieth part of an inch. None of these fibres can be broken across, so as to present an even surface; for they break irregularly, and become divided at the point of fracture into very minute spiculæ, which also assume a somewhat rectangular shape.

THE small spiculæ or fibres are transparent and colourless, with a considerable degree of lustre; but the unbroken part of the stone possesses less lustre than the separate spiculæ, and much less transparency, from a want of compactness, and from the effect of many minute cracks.

Its hardness is not easily determined, on account of brittleness; but when a piece of it is rubbed against glass, though the fibres crumble down very quickly, yet the glass is slightly scratched at the same time. Small fragments of it can be broken with the fingers, or crushed by pressure, into very slender spiculæ, which are sharp, and apt to penetrate the hands when touched. Although the cohesion of its component parts be so weak, yet it bends, and yields in some measure before it breaks, and is not easily ground to powder in a mortar.

I FOUND the specific gravity of different pieces of the specimen, taken in distilled water at the temperature of  $60^{\circ}$ , to vary from 2.643 to 2.740.

THIS stone has the property of appearing luminous in a dark place, both by friction and by heat. A very slight degree of friction produces this effect; for a person can easily distinguish a phosphoric light, even if he draws his finger across it. When struck with a hammer in such a manner that small fragments are driven off, they appear luminous in passing through the air, and continue to shine for a moment after falling on the ground; and

and a hard body drawn over it leaves a track of light, which remains a second or two visible. When a piece of the stone is pounded quickly in a mortar, a strong light is emitted; but after being wholly reduced to powder, it no longer shines.

THE light which it gives by slight friction, is fully equal to that produced by two quartz pebbles, rubbed or struck against each other strongly.

SMALL fragments of this zeolite placed on a piece of hot iron, or of clay, also become luminous, and shine with nearly as much brightness as common blue fluor does, when heated in the same manner. By being made red-hot, however, it is deprived of the property of giving light afterwards by heat, though it still appears faintly luminous by friction. In all these experiments it is a reddish white light which it emits, accompanied at times with reddish momentary kind of flashes.

It can be melted without difficulty into glass. When a small piece of it is heated gradually with the blowpipe, it first becomes white and opaque; a number of fissures are then formed, by which its bulk appears somewhat increased; and after the flame is made sufficiently intense, it melts into a globule of colourless glass, the transparency of which is imperfect on account of many minute air-bubbles. Although thus fusible by strong heat, yet a very low degree of ignition scarcely affects it. A piece of the stone, after having been put into a crucible, and exposed for a short time to a fire which made it just visibly red-hot, was not altered in appearance, and had not lost more than  $\frac{1}{100}$  part of its weight. In a temperature, however, about  $20^{\circ}$  of WEDGWOOD, I found that a fragment of this zeolite became opaque, and more easy to pulverize than in its natural state; at  $90^{\circ}$  it did not melt, but was glazed on the surface, adhered to the crucible, and had begun to sink down; and at  $120^{\circ}$  melted into an imperfect glass of a light greenish yellow colour, deficient

deficient in transparency, which acted strongly on the crucible.

IN temperatures sufficiently high, it loses from  $4\frac{1}{2}$  to 5 *per cent.* of its weight; but to produce this effect, the heat must be equal at least to  $20^{\circ}$  or  $25^{\circ}$  of WEDGWOOD. Part of the volatile matter thus driven off is carbonic acid; for the zeolite, when reduced to powder, and mixed with acids, produces a slight effervescence. I found, by three different experiments, that the loss of weight after the effervescence was about 3 *per cent.*; consequently, the remaining 2, or  $2\frac{1}{2}$  parts, may be presumed to consist of moisture.

THIS substance produces a jelly, as most other zeolites do, with the stronger acids. When ground to powder, and mixed with the sulphuric, nitric, or muriatic acid, the mixture becomes a firm jelly in a few minutes, provided the acids are not much diluted, or used in too great quantity. That which is formed by the action of the nitric or the muriatic acid, is nearly transparent; but the stone contains so much lime, as will presently be shown, that with the sulphuric, the jelly is white and opaque, on account of the sulphat of lime which is generated.

THE prehnite in which it was inclosed, as already mentioned, is of a light green colour, with some lustre, and a considerable degree of transparency. It gives fire with steel, can be readily melted with the blowpipe, and froaths up much before fusion.

HAVING made these preliminary experiments respecting the general properties of the zeolite, I subjected a portion of it in the next place to analysis, to ascertain of what earths it was composed, and found that it consisted almost wholly of silica and lime, with a certain proportion of soda. The following were the methods by which it was analyzed.

1. ONE hundred grains, reduced to fine powder in a mortar of flint, were mixed with 500 grains of muriatic acid, and 1000 grains

grains of distilled water. The powder was immediately attacked by the acid; some heat, and momentary effervescence were produced, and in a few minutes the filix appeared floating in a loose, and very divided state. The mixture being heated on a sand-bath, became in a short time a thin transparent jelly. I exposed this jelly to a gentle heat (less than  $200^{\circ}$  of FAHRENHEIT), till it became nearly dry. Water was then poured on it, and the mixture, after being digested for some time, was filtered. The undissolved part collected on the filter, when sufficiently washed, was dried and heated red-hot for a quarter of an hour. It weighed  $51\frac{1}{2}$  grains, was perfectly white, and proved on examination to be pure filix, unmixed with any other earth.

2. THE filtered solution, which was transparent and nearly colourless, after being saturated with caustic ammonia, deposited a small quantity of a brownish white precipitate. Having collected this precipitate on a filter, and washed it, I exposed it for a few minutes to a red heat. It then weighed one grain, and had become brown, and was found to consist of half a grain of oxyd of iron, and half a grain of argil.

3. IN the next place I added a few drops of sulphuric acid to the solution, to discover if it contained any barytes or strontian. The acid produced no precipitate. The solution was then evaporated to a small quantity, and boiled with carbonate of ammonia. Some carbonate of lime was thrown down, which, after being washed and exposed for a few minutes to a very low red heat, weighed 57 grains. To drive off the carbonic acid, and ascertain the real quantity of lime which this precipitate contained, I put it into a small crucible of Cornish clay, which was inclosed within a Hessian crucible, and heated it for three hours. The fire was raised slowly at first, but was afterwards increased to  $80^{\circ}$  of WEDGWOOD. The lime adhered slightly

slightly, and weighed  $31\frac{1}{2}$  grains. By treating it with sulphuric acid, I detected some slight traces of magnesia, the rest being wholly converted into sulphate of lime.

4. THE different earths having been thus separated from the solution, it was evaporated to dryness, for the purpose of obtaining the soda, which I had learned by previous experiments would be wholly taken up with the other soluble parts of the zeolite, when decomposed by acids. After the evaporation, a white salt remained, which was heated gradually in a crucible to volatilize the muriate of ammonia, formed in the course of the analysis. When white vapours ceased to rise, the heat was increased to redness, and a salt was left which weighed 17 grains. On re-dissolving it in a small quantity of water, and boiling the solution with carbonate of ammonia, a minute portion of carbonate of lime was precipitated, which weighed one grain; equal to about half a grain of pure lime. I obtained the salt by a second evaporation; and after it had been exposed again to a very low red heat, it weighed 16 grains, and consisted wholly of muriate of soda.

ACCORDING to Mr KIRWAN's experiments, 16 parts of muriate of soda in crystals, contain 8.5 parts of soda; but the 16 parts above mentioned, by having been dried in a red heat, and consequently freed from water of crystallization, would contain somewhat more than 8.5 parts of soda. However, the proportion of soda in 100 parts of the zeolite, may be stated at 8.5 parts, which, though probably rather less than the real quantity, must be very nearly correct.

HAVING finished the experiments just described, I analyzed the zeolite a second time; and made use of nitric acid, for the purpose of ascertaining whether any muriatic acid entered into its composition. By the test of nitrate of silver, I found that  
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some traces of the muriatic acid could be distinguished, although the quantity was very small. With regard to the proportion of the earths, the results of the second analysis corresponded almost exactly with the former.

I ALSO exposed some of the zeolite to the action of the sulphuric acid, in the following manner, with the view of obtaining the soda only, the earths being disregarded. One hundred grains\*, reduced to fine powder in the flint mortar, were mixed with 250 grains of sulphuric acid, diluted with twice its weight of water. Some heat and very slight effervescence were produced, and the mixture soon became thick and gelatinous. It was then evaporated slowly to dryness in a sand-bath, in a cup of Chinese porcelain; and the dry mass was pulverized and boiled for half an hour with water, and filtered. Having washed the undissolved residuum sufficiently, I boiled the filtered solution with carbonate of ammonia, which precipitated some earthy matter. After this had been separated by filtration, the solution was evaporated to dryness by a gentle heat; and the saline mass left was put into a crucible, and heated slowly to redness. A white salt remained in the crucible, which weighed 19 grains. To free this salt from any remains of earthy salts which might be mixed with it, I dissolved it in a small quantity of water, added some carbonate of ammonia, and boiled the mixture for a few minutes, by which means a slight earthy precipitate was thrown down. This being separated as before, the salt was again collected by evaporation, and heated to redness. It now

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weighed

\* I MADE use of small quantities only of the zeolite in all these experiments; because, as the specimen appears to be the only one of the kind which has been found, I wished to preserve as much of it entire as possible.

weighed  $17\frac{1}{4}$  grains, and was found on examination to be pure fulphate of soda \*.

By Mr KIRWAN's estimation,  $17\frac{1}{4}$  parts of dry fulphate of soda contain nearly 8 parts of alkali; consequently, from 100 parts of the zeolite there have been obtained by the process last described, 8 parts of soda. This result corresponds with the former, in which the alkali was collected in the state of muriate of soda, as nearly as can be expected in such experiments; and it must be remembered, that the proportions of the component parts of neutral salts are not ascertained with precision.

ACCORDING to the different experiments now detailed, 100 parts of this zeolite contain,

Silex (No. 1.) 51.5

Lime (No. 3. and 4.) 32

Argil (No. 2.) 5.0

Oxyd of iron (No. 2.) 5.0

Soda, about 8.5

Carbonic acid and other volatile matter, 5.0

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with some traces of magnesia and muriatic acid.

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\* THE experiments which shewed that soda was the alkaline basis of this salt, have not been stated here; because I formerly gave a description of the methods used in examining the same salt, in the paper on Whinstone and Lava, published in Part I. of this volume, (p. 85.). To avoid unnecessary repetition, I beg leave to refer to that paper, both with regard to the manner in which I endeavoured to determine the purity of the saline matter, and also that of the silex, and some other earths.



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THE stone which has now been described resembles some of the varieties of Tremolite mentioned by SAUSSURE \*, in the property of giving a phosphoric light by friction. Its specific gravity also is somewhat greater than that of the ordinary kinds of zeolite, as stated by mineralogists. Excepting in these particulars, however, it has the principal characters of a zeolite; for example, in its internal composition, in having been found in a whin rock adhering to prehnite, and in producing a jelly with acids. Tremolites have a higher specific gravity than this stone, are more infusible, and are considerably different in their composition †. Besides, such kinds of tremolite as I have examined cannot be decomposed by acids, even when boiling, and must be heated with potash or soda before their component parts can be separated; but the substance in question is completely decomposed by acids, like the greater number of zeolites, in a very few minutes, and without the assistance of heat. For these reasons, it appears to me to be a zeolite.

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\* *Voyages dans les Alpes*, 1923.

† KIRWAN'S *Mineralogy*, vol. 1. p. 278. *Traité de Mineralogie* par HAUY, tom. 3. p. 151 and 227.



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XIII. DISQUISITIONS *on the ORIGIN and RADICAL SENSE of the GREEK PREPOSITIONS.* By JAMES BONAR, F. R. S. EDIN.

[*Read Dec. 19. 1803, Jan. 16. & Feb. 20. 1804.*]

ETYMOLOGICAL researches have often been branded as trifling, occupying the mind in useless labour, and contributing little to the progress of science, taste, or beneficial knowledge. The justice of this treatment may well be questioned. That this species of study, like many others, has frequently been directed to unimportant objects, cannot be denied ; but it by no means follows, that this is the fault of the subject, or that from such researches, however pursued, nothing valuable can be the result. If by etymological investigations, even in their most limited form, essential aids are obtained for facilitating our acquaintance with the writings of antiquity,—if such inquiries have often proved one of the most certain means for attaining accuracy of language and precision of ideas,—if by following them out on an extensive scale, assistance might probably be gained for elucidating some of the obscurities of early history,—and if from this source valuable materials may at all times be drawn for tracing the progress of the intellectual powers, and unfolding the laws of thought through the phenomena of its expressions,—if these, and perhaps other merits, must be allowed to etymological and grammatical disquisitions, they are surely sufficient to rescue from contempt this class of inquiries. If to all

this we can add, what I believe will pretty generally be found to hold true, that those who affect most to despise this branch of study, have seldom any thing better to substitute in its place, a sufficient vindication will, I hope, be afforded, for employing a little literary leisure in the culture even of this apparently unpromising field.

THESE considerations may, I trust, then, plead my apology for taking up the attention of the Society at this time with some etymological hints and conjectures,—not, I own, directed towards the more extensive tracts in which profounder scholars often wish to range, but confined within the narrow limits of a particular branch of a particular language,—a language, however, justly admired for copiousness and elegance, and a branch of it which at different times has been thought worthy the attention of critics of no inferior note.

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By the ingenious and elaborate researches of recent etymologists, the structure of language has been made to assume a simpler dress than what grammarians had once thought proper to invest it with. The *parts of speech*, as they are technically termed, have been reduced from their numerous classes to two; the connecting particles, usually termed and supposed indeclinable, have been completely excluded from their separate rank; and by tracing out the genealogical relations of words, all have been fairly shown to belong to one or other of the classes of Nouns or Verbs\*.

THE

\* EVEN this classification of words might be still farther simplified, and either the noun or the verb shewn to be the root of all language. Etymologists have differed

THE germ of this theory may be found in the early English etymologists JUNIUS and SKINNER ; but with them it was carried no farther than the analysis of certain individual words, without being advanced to any species of complete and general system. A few of the French grammarians seem to have made some approaches to the same system ; but, it must be owned, that it is to the Dutch etymologists SCHULTENS, TEN KATE, and above all, HEMSTERHUIS, and the disciples of the Hemsterhusian school, VALKNÆR, LINNÆ and SCHEIDE, that we owe the first complete developement of this new and subtle theory of language. The labours of the Dutch etymologists, however, for a long time obtained but little notice : even the system they had thus ingeniously constructed, remained almost unknown beyond the immediate limits of their own school. In France, ANCE DE VILLOISIN appears to have had some imperfect acquaintance with it. In this country, I do not know if it had been heard of at all, till the same theory was exhibited in a no less conspicuous than amusing form, by the author of the *Diversions of Purley*. When, in that lively work, the system was laid down, and at the same time successfully applied to the analysis of many of the English particles, our grammarians and etymologists at length began to question the old system of grammatical arrangement in the classification of words. The new theory gradually gained ground, and now the truth of it seems to be almost generally admitted, though much still remains to be done in the way of its particular application \*.

## SATISFIED

ferred in opinion to which of the two that place is to be assigned. It is unnecessary to enter into that question at present, though I may afterwards take occasion to point out some reasons which induce me to think, that tracing language to its first elements, we shall probably find that it is from the Verb the whole has gradually branched out.

\* THOUGH I have thus stated, what, I think, cannot be disputed, that the theory of HORNE TOOKE, and that of the Dutch etymologists, is essentially the same,

SATISFIED of the truth of this system, and persuaded, that by following it out as far as possible, the best foundation will be laid for a just investigation of the structure of language, it occurred to me, that the general principles thence afforded, might be applied with advantage, as a guide for analysing at least one class of connective particles in a language which appears to offer peculiar facility in such a research. Should this be found to succeed in one language, it must facilitate similar investigations in others, and every fact established in the progress of the inquiry, may be regarded as both illustrating and confirming the general system.

THE Greek language certainly possesses many advantages for such an analysis. It is a language of regular structure; its roots formed within itself, in which, consequently, the original stamina, with their subsequent ramifications, may, by accurate investigation, be satisfactorily traced. The class of connectives in this language upon which this experimental investigation is proposed at present to be made, is the *Preposition*, a class of words of considerable importance in the structure of every language, at the same time so closely connected with nouns, that their mutual relations may be marked with less difficulty than is found in analysing some of the others.

MUCH,

I wish not to be understood as asserting that the one was borrowed from the other. Similar ideas may have occurred to both without any communication. The Dutch etymologists certainly claim the priority in point of time, but the praise of originality cannot upon that account be denied to either. Dr BEDDOES, in a long note appended to his *Observations on the Nature of Demonstrative Evidence*, has taken great pains to prove the originality of HORNE TOOKE. This point need not be disputed; but if it was to be made a subject of controversy, it was surely unnecessary, in discussing it, to throw out, as the Doctor has done, a number of contemptuous, and by no means well-founded sarcasms, in deprecation of the labours of the Dutch etymologists, and disparagement of classical literature in general. Observations of that nature are as little adapted to add force to an argument, as to do credit to the author.

MUCH, I know, has already been done towards facilitating such an investigation. The successive labours, first of Dr MOOR, after him of more recent grammarians, above all the elucidations of the learned and acute Professor of Greek in the University of this City, have both opened the proper track of inquiry, and cleared away much of the rubbish, with which the jejune and trifling *minutiae*, the distinctions without a difference, and repetitions of examples without a connecting principle, introduced by STEPHENS, HOOGEVEEN and VIGERUS, had blocked up the avenues to all such disquisitions. The profounder researches of LINNEP and SCHEIDE have thrown much additional light upon many particulars before involved in obscurity, and their etymological deductions, though in many instances imperfect, contain valuable materials for analysing the real structure and ramifications of the Greek language\*. The track being once marked out, and the path in a great measure cleared, progress becomes comparatively easy; little more is necessary than to follow out the line where those who preceded appear to have stopped short too soon:

IN analysing the prepositions of any language, one general principle may be assumed as certain, That to every preposition

one

\* THE *Analogia Græca* of LINNEP contains a general developement of the system adopted in the Hemsterhusian school. This was followed by the *Etymologicon Linguae Græcæ*, the joint production of LINNEP and SCHEIDE, in which the principles of this analogy are applied for analysing all the primitives of the Greek tongue. The merit of both must be admitted: both will be found worthy the attention of any one who wishes to investigate the subject of Greek analogy. From the latter work, in particular, I have derived much assistance in the present disquisition, though it has frequently proved less satisfactory than might have been expected. The analysis of the words is too general and indefinite: many of the primitive roots are explained as merely denoting *motion*; an idea so vague, that unless limited in some specific mode, it could afford little aid in our progress to a systematic analysis of the principles of the language.

one primary radical idea was originally affixed,—that this idea was for the most part taken from sensible objects,—and that from this radical sense all the secondary applications may either immediately or circuitously be traced. In the investigation of this radical sense of each of the prepositions, I think it will likewise appear, that all of them were originally either nouns or participles, most of them verbal adjectives, at first usually joined with some common substantive to complete the sense; which substantive, by use, came at length to be dropped, as unnecessary to be expressed, being immediately implied and understood. From this it will likewise follow, that in the junction of these words with other nouns, the primitive rule of construction by which they were joined in sentences, was that which is termed the *genitive or ablative absolute*; a construction of sentences, which though stigmatized, and perhaps not unjustly, by Lord MONBODDO, as lame and gaping, yet was probably of extensive use in the early stages of human speech, when bare co-existence of phenomena or events, (the precise idea denoted by this mode of construction), was more attended to than that mutual relation and dependence, the gradual discovery of which afterwards gave rise to more compact and connected forms of expression.

PROCEEDING upon these principles, we may possibly be able to discover the radical sense of each preposition, as well as some of the leading circumstances by which, in time, many of them came to be applied, to denote ideas apparently remote from their original meaning. The minuter ramifications to which these significations afterwards branched out, it would lead far beyond the bounds of a single paper to attempt investigating.

IN conducting such an analysis, each preposition must be separately examined,—a minute and tedious process, perhaps, but the only one by which the object can be fairly accomplished. No particular arrangement being at all necessary for our purpose,



I shall be satisfied to take them as they occur in the order of the alphabet.

Ἀμφι.

THE near relation which this preposition bears to the adjective ἄμφω, *both*, and the adverb ἀμφίς, *on both sides*, evidently points out an identity of origin as well as affinity of meaning. All three appear to be immediately derived from the obsolete verb ἄμω, to *embrace* or *grasp* \*. From ἄμω it is probable came the verbal adjectives ἄμος and ἄμης, signifying *grasped* or *embraced*. From many examples in the more ancient Greek writers, it appears to have been a frequent practice either to add to words the termination φι, or simply to insert the letter φ before the termination. Of this last, the instances are not unfrequent in HOMER and HESIOD, as ὄχρεσφι for ὄχρεσι, μέσφα for what appears to have been originally μέσα, and ἐννηφι for what I should suspect had been originally ἐννης or ἐννη †. By a similar insertion, ἄμος and ἄμης became ἄμφος and ἄμφις, both of them signifying, as before, *grasped* or *embraced*. Ἀμφω, the dual number of ἄμφος, thus exactly expressed *two objects, grasped, embraced or united*, the precise idea denoted by the term *both*. To ἄμφις, expressing *grasped*, some common word denoting *place, object*, or the like, being at first usually joined, and afterwards in common use omitted, as being universally understood, ἄμφις and its oblique case ἀμφὶ came to signify literally *place or object grasped, embraced, or comprehended*.

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\* THE root of the common Greek verb ἀμῶν, to *reap*, (literally to *gather together* the fruits of the earth), and of the Latin verb *amo*, to *love*, originally denoting to *grasp at* or *strive to obtain*.

† PERHAPS the φ thus inserted was originally the Æolic digamma; a letter common in the most ancient Greek writings, and it seems probable, pronounced not unlike the φ.

HENCE the different applications of ἀμφὶ are easily deducible.

1. ABOUT or round a place or object.

Ἀμφὶ πόλιος οἰκίουςι, (HERODOT.) “they dwell—place grasped  
“or comprehended by their dwellings—the city,”—“round  
“about the city.”

—— ἀμφὶ δὲ ποσσὶν ἔχε πτερόεντα πῖδιλα, (HESIOD.) “and  
“had the winged sandals,—object grasped by them—his feet,”  
“—round his feet.”

—— ἀμφὶ δ' ἄρ' ἁμοισιν ἐάλετο ξίφος. HOMER.  
“And threw the sword,—place grasped or embraced by the  
“sword-belt,—his shoulders,”—“round his shoulders.”

Ὡς δ' ὅτ' ἐν ουρανῷ ἄστρα φαεινὴν ἀμφὶ σελήνην  
Φαίνεται ἀριπρεπέα. HOMER.  
“As in heaven the splendid stars appear,—object included or  
“comprehended in their line of appearance,—the refulgent  
“moon,”—“round the refulgent moon.”

SOMETIMES, instead of directly round about, ἀμφὶ, by an elliptical mode of phraseology, expressed only near about :

—— ἤριπε δ' ἀμφ' αὐτόν,  
“he fell—place of falling—the space including or comprehend-  
“ing him,”—“near about him, or close beside him.”

2. AMONG.

—— ἀμφὶ σφίσι πένθος ὀρώρει. QUINT. SMYRN.  
“sorrow arose—objects included in its sphere of action,—ob-  
“jects embraced in it—them,”—“among them.”

3. UPON.

—— καθεσάλεν ἄνδρα κατὰ χθονός, ἀμφὶ δ' ἄρ' αὐτόν  
Εἴετο. QUINT. SMYRN..  
“He threw down the man to the ground; and sat,—object  
“grasped by his sitting—him,”—“upon him.”

4. FIGURATIVELY—about or concerning.

Ἀμφὶ ἄστων γράφη. LUCIAN.  
“a treatise—subject comprehended—the stars,”—“concerning  
“the stars.”

And

AND immediately related to this meaning,—FOR or ON ACCOUNT OF.

ἱππῆες ἔχον πόνον, ἀμφὶ δ' ἀέθλοις  
 Δῆριν ἔχον καὶ μόχθον. HESIOD.

“The horsemen were labouring and in earnest contention—object grasped in their contention—the prizes;”—“object in which their contention centred—the prizes.”

Ὡς δὲ λέοντες δύο ἀμφὶ παμένης ἐλάφοιο  
 Ἀλλήλοις κοτέοντες. HESIOD.

“As two lions irritated at each other—object grasped in their irritation—object in which their irritation centered—a slain hind.”

IN every one of these, as well as in all its various applications, we find *ἀμφὶ*, a noun, assuming, from its use, the character of what is termed a preposition.

*Ἀμφὶ*, so far as I can find, is a preposition either not at all or very seldom to be met with in the writings of the Greek geometers, *περί*, being the word generally made use of by them for *about*. The reason I take to be, that this science, relating chiefly to lines and surfaces, *ἀμφὶ*, which did not immediately, but only indirectly, denote these, was less proper for their purpose than *περί*, which, as we shall afterwards see, literally and radically meant *a boundary*.

THIS preposition *ἀμφὶ* has not been naturalized in Latin\*; instead of it *circa* and *circum* came into use, evidently the feminine and neuter genders of the old adjective *circus*, the masculine of which we find in common use as a substantive noun, denoting a *round body*, the primitive of *circulus* a circle. *Circa* and *circum*, therefore, properly denote *a line going round a place or object*.

S s 2

Here,

\* EXCEPT in the inseparable preposition *am*, denoting *about*, a mutilation of *ἀμφὶ*, or rather of the original word before the *φ* was inserted.

Here, as in many other instances, we find an example of an idea expressed with equal propriety either by a noun or its correlative. The Greeks, in using *ἀμφι*, kept in view that an object was included; the Latins, in using *circa* and *circum*, and the Greeks themselves, too, in using *περι*, had in their eye that there was a boundary containing.

#### *Ἀνά.*

*Ἀνά* is one of those prepositions which, partly from the obscurity of its primitive root, and partly from its varied and somewhat anomalous mode of application, presents considerable difficulty in the investigation of its primary and radical sense. LINNÆUS supposed it to mean *pressure upward*; an idea which it certainly does very frequently denote, but into which all the others cannot be fairly resolved. In Professor DALZEL's *Fragmenta*, the proper signification of it is said to be *per, through*, in which opinion STEPHENS coincides. But to this idea, though so respectably supported, I cannot altogether accede. *Through* is undoubtedly one, and a very common meaning of *ἀνά*, but, like the former, can only be ranked as one of its secondary applications. Dr MOOR, I apprehend, has come nearest the true radical signification of *ἀνά*; he explained it as expressing "*the lines of direction traced backwards*;" a sense not very remote from what appears to be the original one.

*Ἀνά*, in the immediate form of a noun, does not, I believe, occur in the Greek writers now extant; but from its cognates and derivatives, it is not difficult to discover the traces of its former meaning. *Ἀνά*, as an adverb, is common, expressing *a circumstance far back,—removed backwards* from us, whether in place or time: *αν* occurs in composition in a number of words, with the sense as it were of *contradiction* or *reversal* of the main idea, as *ἀνάνδρως*, *unmanly*, *ἀνόσιος*, *impious*, *ἀνόρατος*, *invisible*: *ἀνευ*, a derivative from the same root, generally carries a similar force of re-

versing

versing the idea denoted by the word with which it is connected.

In all these different cognates, we find the idea of *reversing* or *tracing backward* implied; and APOLLONIUS SOPHISTA, in his *Lexicon Homericum*, mentions this as the primitive idea denoted by the preposition ἀνά \*. Here, then, I think, we may fix the radical sense of the word. Ανά appears evidently to be a noun, having the literal signification of *reversed*, *turned back*, or *traced backwards*; and, if necessary, some common and obvious word, such as *direction*, *position*, or the like, may be supposed to be implied and understood; but this will seldom be required †.

WE may, therefore, give the radical meaning of ἀνά as *back*, *backward*, *reversed*, or *traced back*. Thus, in XENOPHON, ἀνά ποταμὸν διεκομίσαν, “they bore them,—the river being traced backwards in their bearing,” i. e. bore them against the stream, or up the river.

FROM this radical meaning, the other applications will be found naturally to arise.

I. VERY frequently ἀνά signifies *up* or *upward*, because *down* or *downward* being the natural and usual direction which bodies take when left to themselves, *up* is consequently the natural direction reversed or traced back, and therefore properly expressed by ἀνά.

Ὡς εἰπὼν ἀνά νηὸς ἔβην, ἐκέλευσα δ' ἑταίρους·

Ἄυτούς τ' ἀμβλαίνειν, ἀνά τέ πρυμνήσια λῦσαι. HOMER.

“Having thus spoken, I ascended the vessel, and ordered my  
“ companions

\*. Ανά. Κυρίως μὲν ἡ πρόθεσις, ὡς ἐν τοῖς

Ἀφ' ὧν μοι μύροντά ἰδὼν, ἀπὸ τ' ἰδραμ' ὀπίσσω.

APOLLON. SOPHIST. sub voce Ανά.

† PARKHURST's idea of the derivation of this preposition from a Hebrew word signifying *an answer*, coincides pretty nearly with the idea here stated. The Hebrew word for an answer may probably have originally denoted *a rebound*.

“companions to go up (or on board) and to unloose the “cables;”—I went—direction of my going—the vessel—in the order of position the reverse of what is usual with bodies, *i. e.* upwards; I ordered my companions to loose the cables—line in which they loosed them—tending upwards, or reversing the natural tendency of bodies.

————— ὑψὸς ἀείρας  
Θῆκεν ἀνὰ μυρίην. HOMER.

“Having raised them aloft, he placed them on a tamarisk,”—placed them—point of placing them—a tamarisk;—situation—the reverse of what is the usual tendency of bodies,—that is, *up* or *aloft*.

2. As the notion of a return or reversal always of necessity presupposes the idea of a prior movement or position to which this is opposed, ἀνὰ, from expressing only the second, came naturally to be extended to denote both together. Hence we often find it applied to express the idea of *backwards and forwards, up and down*; that is, course backward, proceeded by course forward, course upward proceeded by position downward, or, as justly and accurately stated in Mr DALZEL’s *Fragmenta*, “*motum huc et illuc—itionem et reditionem—sursum et deorsum.*” *Ἀνὰ τὰ ὄρη πλαναῖσθαι* (XENOPH.) “to wander—course of wandering—the mountains;—first forwards—then this reversed or “traced backwards,”—hence up and down the mountains,—*over the mountains.*

————— μνησάθε δὲ θούριδος ἀλκῆς  
Νῆας ἀνὰ γλαφυράς. HOMER.

“Keep in mind your impetuous courage—by the hollow ships,”—going along the ships—backwards and forwards—first forwards,—then that course traced back again.

IT

It is from this application that *ἀνά* has often come to be used for *through* or *over*,—through the mountains, or over the mountains, equivalent to up and down—backward and forward.

3. *Ανά* is joined with nouns of number, to denote *regular distribution*. Στρατιώτας ἐξίεναι κελεύει ἀνά πέντε, (VIGERUS), “the soldiers to go out by fives,”—to go out five—then traced back again,—to go out till they came to five, and then begin or trace back the reckoning again. Κιννάμωμου καὶ νάρδου ἀνά οὐγκίαν μίαν, (VIGERUS), “of cinnamon and nard, an ounce each,”—of cinnamon an ounce,—then trace back the same reckoning for the nard.

4. SOMETIMES, though but rarely, *ἀνά* is used to denote *according to*. Λαλεῖν ἀνά τὸν αὐτὸν λόγον (CLEMEN. ALEX.) “to speak—the same word traced backward,”—according to the same word.

UNDER these different heads, all the applications of *ἀνά* may, I should think, either directly or indirectly be arranged. In all of them we are warranted in regarding it as a noun; and though it cannot be denied, that the varied application and obscure origin of this preposition may sometimes occasion difficulty in marking its exact progress or ramification, this does not at all detract from the truth of the general proposition. Here, as in many similar cases, the following observation of LINNEÆ will be found to hold good: “It is not to be expected, that the analogy of any language can be perfectly traced, from the want of many primitive words, which, having gone into disuse, are not now to be met with, as the most ancient compositions in the language have probably perished. This defect is still increased by the carelessness of lexicographers, who have not been attentive even to preserve the old words that were to be met with at the time they wrote.” Hence, under every root, we need not be surprised to find blanks in the analogy which no ingenuity or conjecture can now possibly supply.

Αντί.

## ΑΝΤΙ.

ΑΝΤΙ is explained both by LINNÆ and SCHEIDE, as one of the cases of an obsolete noun *ανς*, in the genitive *ἀντος*, signifying *front* or *face*. In this etymology they appear to be well founded; for though the noun *ανς* itself is fallen into disuse, yet we still find sufficient remains to indicate its former existence. *Αντα*, evidently its accusative case, is in frequent use as an adverb, to express *before*,—*in presence*, *in face of*: *ἀντην*, probably the accusative case of a cognate noun, is likewise used adverbially in the same sense with *ἀντα*: the adjectives *ἐναντίος* and *ἀντίος*\*, and the adverb *αντικρυς*, both of them immediate derivatives, express also *in presence*, *in front of*: the verb *ἀντιθεῖν* is explained by HESYCHIUS as synonymous with *λιτανεύειν*, to supplicate, *i. e.* to *fall down before*, or *in front of*, *in presence of*. To all this may be added, that the Latin preposition *ante* (evidently the same word radically with the Greek *ἀντι* †), uniformly expresses *before*, equivalent to *in front*; and in the same language we find a substantive noun *antæ*, (a plural from the singular *anta*,) denoting the

\* THAT *ἀντίος* signifies literally *in front*, the following trait in the Spartan epigram, which describes the body of THRASYBULUS carried back from the battle, clearly ascertains:

Ἑπτὰ πρὸς Ἀργείων τραύματα δευζόμενοι;  
Διπλὺς ἀντί τι πάντα.

“having received seven wounds,—showing them all *in front*.”

† THE Latin language is well known to be derived from the Greek; but it should always be attended to, that it is the *Æolic* dialect of the Greek which must be regarded as the immediate parent of the Latin. Hence many of the words transplanted from the Greek to the Latin are found to differ from the common dialect, but to resemble very closely the corresponding word in the *Æolic*.



the posts *set up before the entry*, that is, the *fronting part* of a house.

*FRONT* or *face*, then, appears to be the radical sense of *ἀντί*.

Ἰσάμενος ἀντὶ θύρης, “set or placed—front the door,”—fronting the door—before the door.

Ἀντ’ ἡελίοιο τετραμμένος, (HESIOD), “turned—front the sun,”—fronting the sun\*.

THOUGH *front* was the original and radical meaning of *ἀντί*, yet it is to be observed, that the immediate and direct use of the word, in this sense, seems to have been in a great measure superseded by some other prepositions. It is in the secondary applications, chiefly, arising out of this primary idea, that we find *ἀντί* employed by the Greek writers. These secondary applications are principally four,—*opposition*, *comparison*, *preference* and *substitution*.

1. *OPPOSITION*,—because fronting or facing is both the natural attitude of direct hostility, and the most obvious mode of retarding or counteracting what we wish to prevent.

Ἄντ’ ἀνδρὸς ἵτω, (HOMER), “go—object fronting or meeting you in face—the man,”—against the man;” ἀντιλέγω τοῦτον λόγον, “I contradict that speech,”—“I speak in face of that speech.”

2. *COMPARISON*,—because placing objects so as to make them front or face each other, is the most common and ready mode of enabling us to compare and contrast them. Ἀργυρίον ἀντὶ βοῶν, “silver compared with oxen,”—silver—object put in front or contrasted—oxen. Βασιλεὺς ἀντὶ μυρίων ἔσι στρατιωτῶν,—“a king is comparable to ten thousand foldiers,”—“a king is—object set in front to be compared—ten thousand foldiers.”

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T t

3. *PRE-*

\* In this phrase, which occurs, Ἔργα καὶ Ἡμίραι (line 725) it is plain that *ἀντί* can mean nothing but *in front*.

3. *PREFERENCE*,—because what is most valuable is conceived and spoken of as occupying the front or foremost place. *Ἀντὶ χρημάτων ἐλέσθαι τὴν δόξαν χρῆν*, (ISOCR.) “glory should be taken in “preference to riches,”—glory should be taken as an object, holding the place in front of, or in a more honourable rank,—than riches. *Ὅν ἐγὼ φημί ἀντὶ παντῶν τῶν ἐνθάδε ἀγῶνων εἶναι*, (PLA-“TO), which I aver to be the best of all contests,”—which I aver to be a contest—holding the front or first rank of all contests.

---

κάλλος,  
*Ἀντ' ἀσπίδων ἀπασῶν,*  
*Ἀντ' ἐγχέων ἀπάντων.* ANACR.

“Beauty—preferable to all shields—preferable to all lances,”—  
 “beauty—holding a rank in front of, or before all shields and  
 “all lances\*.”

4. *SUBSTITUTION*,—for the reason justly stated in Mr DALZEL'S *Fragmenta*; “verifimillimum est ἀντὶ primitus usurpatum fuisse “ad designandum alterum poni *contra* alterum, id quod fieri so-  
 “lebat antiquissimis temporibus, quum merces essent commu-  
 “tandæ, unde facillime transferebatur ad indicandum ipsam  
 “mercium commutationem vel quodlibet suffectum in locum  
 “alterius.” *Ἀντ' ἀγάπης μῖσος εὕρεσθαι*, “instead of, or in return  
 “for love, to find hatred,”—to find hatred—when the object put  
 in front of it was love. *Εἰρήνη ἀντὶ πολέμου* (THUCYD.) “peace in-  
 “stead of war,”—peace—object set in front, for which it is to be  
 taken in exchange—war.

FROM this last application of *ἀντὶ*, it has come to be generally  
 used by the Greek writers in speaking of commodities sold, to  
 express their price. Examples are unnecessary.

## 5. FROM

\* Such phrases are frequently resolved, by supposing *ἀντὶ*, when it so occurs to  
 denote *comparison* or *contrast* only: but this appears to be insufficient; for though  
 all preference implies contrast, all contrast does not imply preference.

5. FROM a particular view of the primitive idea of fronting or facing, *ἀντί*, when both preceded and followed by the same noun, came sometimes to denote *addition* or *accumulation*; because, when two quantities of the same commodity were placed fronting each other, this could not be for the purpose of exchange, but rather that the one should be added to the other, to complete a mass. Hence such phrases as *ἀνίας ἀντί ἀνίων*, “for—rows—in front—sorrows,”—sorrows added to sorrows,—accumulated sorrows; *χάριν ἀντί χάριτος*, “grace—in front—grace,” grace added to grace,—abundant grace. In English we have adopted a somewhat similar idiom, only instead of employing the word denoting *front*, we use its correlative *after*,—saying “day *after* day,”—“grief *after* grief.”

### Ἀπό.

THE Greek preposition *ἀπό*, and our English preposition *from*, are universally allowed to be nearly equivalent in sense; and probably it may be found, that the origin of both has been in a somewhat similar line. That *ἀπό*, like other prepositions, was originally a noun, there can be little doubt; evident traces of the cognates and derivatives of this noun being still to be met with. Thus, besides the preposition *ἀπό*, we find in use *ἀπό* as an adverb, denoting *far off*, with its derivative *ἄπωθεν* \*, *from far*; and though the adjective *ἄπος*, *distant* or *remote*, appears to have become obsolete, yet its comparative *ἀπώτερος*, *farther*, and superlative *ἀπώτατος*, *farthest* or *most remote*, are frequently found. The word *ἄπος*, likewise as a substantive nouns, occurs once, and, I believe, but once, (EURIP. *Phæniſſ*), and the lexicogra-

T t 2

phers

\* Ἀπό, —ἀντί τῷ ἄπωθεν.

“ Πολλὰν γὰρ ἀπο πλυνείῃσι πόλιος.

“ For the cisterns are at a great distance from the town.”

APOLLON. SOPHIST. in voce.

phers seem at a loss what to make of it. The Greek scholiast conjectures it to have been equivalent to ὕψος, *height*, and thence transferred to denote μήκος, *length*, a strange circuitous mode of application, much more naturally resolved by understanding the noun ἀπὸς as a cognate of the adjective ἄπρος, and hence immediately denoting length or distance. It may be added, that there is in common use a verb ἄπτω, to *tie*, and ἄπτομαι, to *touch*: the root of both these, I should conjecture, must have been a verb ἄπτω or ἄπω, signifying to stretch, as it is not easy, except upon this supposition, to reconcile the different meanings of the active with those of the passive or middle form of the verb.

THESE different cognates, then, seem all to point to the primitive meaning of ἀπὸ, and mark it out as an adjective denoting *distant* or *remote*. Some common noun denoting place or point, being, as in other cases, understood or implied, ἀπὸ came to be used in the literal radical sense of *distant point*,—*remote point*. From being thus frequently used to express a point, more remote than any other in view, the signification was extended to denote likewise the *extreme point*.

THIS primitive radical sense ἀπὸ retained in a number of cases; but upon this primitive meaning there came, it should seem, to be very soon engrafted a secondary application, which in time, in great measure, superseded the former. In any course or progress, it is evident, that, in the nature of things, either the commencement or the conclusion may be reckoned the extreme or remotest point of the line; but in the common use of language it will be found, that it is the former which most generally and readily occurs as the point most remote from the speaker. Hence the *most remote or extreme point*, is often considered as the same with the *point of departure*; and the word which at first denoted the one, came readily to be applied to express also the other. Such appears to have been the case with

ἀπο among the Greeks. From expressing originally the most remote or extreme point only, it came to be most generally used to signify, what in the language of navigators is called the *point of departure*, and hence, by an easy transition, the *point of origin*, the *point of commencing*.

AGREEABLY to this idea, all the uses of ἀπό may be resolved, by a reference either to the primary or to the secondary application of the word.

WE find ἀπό, then, as signifying *from*, applied,

1. To *space* or *corporeal objects*. Ἀπὸ δοθέντος σημείου γραμμὴν ἀγαγεῖν, (EUCLID.), “to draw a line from a given point,”—“extreme or remote point of drawing,”—or, “point of departure or commencement of drawing it,—the given point.”—Ὡρμήατο ἀπὸ Σάρδεων, (XENOPH.), “he marched from Sardes,”—“extreme point,—or point of his departure,—Sardes.” Ἀπὸ τῆς τέχεος μάχεσθαι, “to fight from the wall,”—“extreme point of fighting,”—or “point of outset of the fighting,—the wall.” Ἀπ’ ομμάτων Βαλὼν με, (ANACR.) “darting at me from the eyes,”—“extreme point of darting,—the eyes.”

2. To *time*. Ἀπὸ τῆς νῦν πειρασόμεν, “from the present time we shall endeavour,”—“we shall endeavour,—point of outset of our endeavours,—the present time.” Ἀπὸ δὲ τῆς παλέμου τῆς τῶν ποινῶν διοικήσεως ἑαυτὸν ὑπεξείλετο, “immediately after the war, he withdrew from public business,”—“he withdrew from public business,—point of outset of his withdrawing,—or extreme point of his withdrawing,—the war.” Ὁ Σάβινος κρηφότερος ἀπὸ ἐτῶν ἑπτὰ ἐπιτήδειος, (GALEN), “the lighter Sabine wine is fit for use from seven years old,”—“the wine is fit for use,—point of outset of this fitness,—seven years.” Ἀπὸ σημείου ἑνὸς ἐπιστρέψαι τὰς ναῦς, (THUCYD.), “to turn the ships at one signal,”—“to turn the ships,—point of outset of the turning,—one signal.”

3. To

3. To the *relations* of objects, and chiefly to their relations as effects from a cause either producing or modifying them. Αισθόμενος ἐν ἀπὸ τῷ βελτίστῳ ταῦτα πράττοντα τὸν ἀδελφόν, (DION. HALIC.), "perceiving that his brother was acting thus, not from "the best disposition,"—"acting,—point of outset,—or origin "of acting,—not the best disposition." Δοκεῖ ἐτέρως δὲ τοῖς ἀπὸ τῆς σοᾶς, ἐτέρως δὲ τοῖς ἀπὸ τῆς ἀκαδημίας, (LUCIAN.), "it appears in "a different light to the stoic and the academic philosopher," "—to persons from the porch and from the academy,"—"persons proceeding,—point of departure,—the porch and the academy."

4. IN all these different applications of ἀπὸ, which are the most common, the secondary sense appears to predominate; but ἀπὸ is sometimes found in use, where the original meaning of *remote* or *distant point* directly prevails. This takes place in regard to material objects, in such phrases as the following: Ὡς μὲν τοί γε ἀπὸ τῆς γῆς ἐγένοντο, (XIPHIL.), "when they arrived at a distance from land,"—"when they arrived—remote or distant point,—the land."

———— ἀπὸ χθονὸς εὐρυοδείης  
Ἐς δίφρον θῆκαν. HESIOD.

"They placed him in the chariot away from the earth,"—"placed him—distant or remote point,—the earth."

———— ἀπὸ δὲ γλαυκῶπις Ἀθηνῆ  
Ἐγχεος ὀρμὴν ἔτραπ' ὀρεζαμένη ἀπὸ δίφρου. HESIOD.

"MINERVA turned away the force of the spear,—warding it "from the chariot,"—"turned the force of the spear,—warding "it,—so as to be remote from the chariot,—remote or distant "point,—the chariot."

IT is in this manner that ἀπὸ is likewise used to denote a *negation*; the one object being supposed to be set at such a distance

stance from the other, that they cease to be in any manner connected. *Απείδειξεν ὅτι παντάπασιν αὐτὰ ἀπὸ θυμῆς*, (THEMIST.) “he shewed that it was altogether contrary to his desire,”—“showed that it was so constituted, that it was a point entirely distant or remote in respect to his desire.” *Ὁὐκ ἀπὸ γνώμης ἦν*, (JULIAN. AUG.), “it was not contrary to his mind,”—“it was not in the situation that it was a remote or distant point,—in regard to his sentiments.” *Ὁ ἀπὸ τῆς πρεσβείας*, (VIGERUS), “one who has finished his embassy,”—“who is no longer an ambassador,”—“who is now in such a state as to be remote or distant in respect of the embassy.”

THE Latin prepositions *a*, *ab*, it is well known, are merely the preposition *απο*, in a mutilated form; the radical meaning is nearly, if not entirely the same, being sometimes *remote point*, when *a* or *ab* is used to express *from*; sometimes *point of departure* or *outset*, when used to signify *by*. *Abs*, which has the same sense with *a* or *ab*, is the adjective *ἄπρος* in a contracted form. *Absque*, which is just *abs* with the relative subjoined, takes the sense of *remote or distant point* only, and hence signifies *without*. Thus, “*epistola absque argumento*,”—“a letter without an argument;”—“a letter,—remote,—in respect to which is argument.” “*Absque te victoria esset*,”—“the victory would be (obtained) without you,”—“the victory,—you being remote in respect to which,—though you were remote or at a distance,—would be obtained.”

IN these different senses of the Greek preposition *απο*, we trace a very similar analogy with what appears in the corresponding English preposition *from*. This English preposition HORNE TOOKE will have to be the Saxon *frum*, *beginning*. But from some of the very examples given by himself, and still more from others that might have been adduced, it is easy to see how forced and unsatisfactory the analysis of many sentences on this supposition must be. A different origin must, therefore, be sought

sought for the word. *From*, in Saxon, is *fram* and *fra*. This last appears plainly the root, and it is as evidently a descendant of the Gothic *fairra*, from. Now this Gothic *fairra* was undoubtedly also the root of our adjective *far*, equivalent to distant or remote; and from this root proceeds likewise the German *Fremde*, a stranger. Here, then, we have the same analogy which has been already traced out in the Greek; the root of our English *from* being an adjective expressing *remote*, and the preposition of course meaning just—*remote point*; or *distant point*.

It may be added, upon the subject of  $\alpha\pi\acute{o}$ , that this preposition, or rather its aspirated form  $\alpha\phi$ , is, according to the best etymologists, the root of the English adverb *off*, originally *aff*; and the precise meaning of this is *distant* or *remote*,—*off*, as is well known, being quite a different word, and from a different source, from the preposition *of*, a word radically and properly denoting *class*, *kind* or *species*, though the two have not always been sufficiently distinguished by lexicographers.

#### Διὰ.

THERE can be little doubt, I think, that the preposition  $\delta\iota\alpha$  must have been closely related to the verb  $\delta\acute{\iota}\omega$ . Now  $\delta\acute{\iota}\omega$  in common use signifies to *drive out*, or *drive away*. From many of its cognates and derivatives, however, it is evident, that it must have signified also to *divide*, to *split*, or to *cleave*. The original radical idea expressed by it, therefore, from which both these took their rise, may fairly be conjectured to have been to *bore*, to *pierce*, to *penetrate*. From this verb  $\delta\acute{\iota}\omega$  appears to have come a verbal adjective  $\delta\iota\omicron\varsigma$ ,  $\delta\iota\alpha$ ,  $\delta\iota\omicron\nu$ , signifying, from the verb, *pierced* or *penetrated*, sometimes *DIVIDED*; and, when applied to surfaces or spaces, by a very natural and obvious restriction, *passed over*, or *crossed*. To the neuter plural, or feminine singular of this adjective, some common word, such as  $\gamma\epsilon\alpha\mu\acute{\mu}\eta$ ,  $\chi\acute{\alpha}\rho\alpha$ ,  $\sigma\eta\mu\epsilon\acute{\alpha}$ , or the like, being at first usually joined, as in other cases, and afterwards



wards in practice omitted, *διὰ* came to be used alone for *line*, *space*, *point* or *object* *pierced*, *penetrated*, *divided*, *passed over*, or *crossed*.

FROM this the various applications of the word, as it appears in the form of a preposition, took their rise.

*Διὰ*, as applied to *space*, was used to denote :

1. and most usually, THROUGH.

Πέλεκυς εἰσι διὰ τῷ δένδρει,—"the axe goes through the tree,"  
 "—the axe goes,—*object penetrated,—the tree.*" Κύκλος κύκλον  
 διὰ τῶν πόλων τέμνει, (THEOD. SPÆR.), "Let one circle cut another  
 "through the poles,"—"points *pierced or penetrated in cutting,—*  
 "the poles." Ἐπίπεδον ἐκβεβλήμενον διὰ τοῦ κέντρου τῆς γῆς καὶ τοῦ  
 ἡλίου, (ARCHIMED.), "a plane drawn through the centre of the  
 "earth and the sun,"—"drawn,—points *pierced or penetrated in—*  
 "drawing it,—the centre of the earth and the sun."

———— αἶγλη δι' αἰθέρος ἴκε. HOMER.

"The gleam came through the air,"—"medium *penetrated in its*  
 "coming,—the air."

2. ACROSS.

Δι' ἁλὸς φέρονται,—"they advance across the sea,"—"they  
 "advance,—*object or place passed in advancing,—the sea.*"

"Ὅς τε διὰ Θρήκης ἱπποτρόφος ἐυρέϊ πόντῳ  
 Εμπνεύσας. HESIOD.

"Which blowing through Thrace, famed for rearing horses,—  
 "on the wide sea,"—"blowing,—*place passed or crossed in blow-*  
 "ing, in order to reach the sea." Ἐπορεύετο δι' Αἰγυπτίας εἰς Λιβύην,  
 "—he marched through Egypt to Libya,"—"marched,—*place*  
 "crossed or passed in marching to Libya,—Egypt."

THE only difference between these two applications of *διὰ* consists in this,—that, in the former, the internal part of the object

is understood to be penetrated or divided, in the latter, the surface only; but the idea of penetration is uniformly adhered to.

### 3. BETWEEN.

Διὰ χειρὸς ἔχειν τὰς ἡνίας, (PLUTARCH), "to hold the reins in his hand, that is, between the two parts of his hand,"—"to hold the reins,—*object divided, or, as it were, penetrated by the reins,—his hand.*"

———— εὖτ' ἂν πρῶτα φύγῃς ὁλοῦς διὰ πέτρας. APOLLON.

"When thou art passed safe between or through the destructive rocks,"—"objects as it were divided or penetrated in passing,—the destructive rocks."

### 4. DISTANCE OR INTERVAL.

Ἴππος διὰ τριῶν στάδιων ἐτρέχε, "the horse run to the distance of three stadia,"—"the horse run,—*space penetrated or divided in running,—the extent of three stadia.*" Εξ τῶν πύργων ὄντων δι' ὀλίγου, "from the towers which were at a small distance from each other,"—"towers situate,—*space penetrated or divided by the line reaching them,—a small space,—a small interval.*"

Διὰ was applied likewise to time as well as to space. In this mode of application, it expressed sometimes *through*, sometimes *after*; that is, the action was considered as either actually passing, or as having just passed, an interval of time. The analogy between space and time is so obvious, that the reason of using διὰ is this way can require no elucidation. As referring to time, then, διὰ denotes:

#### 1. THROUGH.

Σκόπειν διὰ νυκτὸς καὶ ἡμέρας, (PLATO), "to deliberate through night and day,"—"to deliberate,—*interval or space of time passed in deliberating,—night and day.*"

Διὰ

Διὰ νυκτὸς ἐγκοιεύδων.

ANACREON.

“Sleeping through the night,”—“time passed in sleeping,—the night.”

## 2. AFTER.

Διὰ δυῶν ἡμέρων ἀποχέοντες τὸ πρῶτον ὕδωρ, (DIOSCOR.), “pouring off the first water after two days,”—“pouring off the water, —interval of time passed before pouring it off,—or passed in its previous state,—two days.” For it is to be observed, that when διὰ is thus made to denote *after*, it always implies, that the state of the transaction then existing, is the result of the preceding action, with which διὰ is joined. Διὰ τριῶν ἡμέρων ἦλθε, “after three days he came,”—“interval passed in coming,—“three days,”—intimating that three days were spent in travelling; whereas, had μετὰ been used for *after*, no such previous connection would have been implied; the three days might have been occupied with an action bearing no relation to his coming.

By a second remove from the primitive meaning of the word, διὰ came also to be applied to express CAUSATION in all its forms. In this mode of application, it appears to have been extensively employed to mark, indifferently, the *efficient*,—the *instrumental*,—the *formal*,—the *material*,—the *procuring*, and the *final* cause. The idea, in all of them, was evidently taken from the use of διὰ to express an interval passed, an intervening object penetrated, or an intermediate space crossed. A cause, of whatever kind, naturally presented itself to the mind as a passage or medium between the bare conception or design of an action and its entire completion. An example of each species of cause, will show, in this use of διὰ, the constant reference to its radical meaning.

THE *efficient*. Δι’ οὗ ἐλάβομεν χάριν, (Rom. i. 5.), “through whom we received grace,”—“we received grace,—medium of its conveyance to us,—him.”

Τοὶ μὲν δαίμονες ἐῖσι Διὸς μεγάλα δια βελάς. HESIOD.

“ They become demons through the will of mighty Jove,”—  
“ they become demons,—medium by which this is effected,—the  
“ will of Jove.”

Δι' ὃν ευνάζετ' ἀνία. ANACR.

“ Through or by which sorrow is lulled to rest,”—“ sorrow is  
“ lulled to rest,—medium of doing this,—it.”

THE *instrumental*. Ὦλετο διὰ πυρός, “ it was destroyed by fire,”  
“ —it was destroyed,—medium passed in order to its destruction,  
“ —fire,”—or, “ medium of conveyance for the destructive ener-  
“ gy,—fire.” Κινεμένης τῆς ἄμμου διὰ τὸ μέγεθος τῶν πνευμάτων,  
THEOPHRAST.), “ the sand being agitated through or by the vio-  
“ lence of the winds,”—“ the sand being agitated,—medium or  
“ passage to its agitation,—the violence of the winds.”

THE *formal*. Τορνεύεται καὶ γλύφεται διὰ τὸ μαλακόν, (THEO-  
PHRAST.), “ it is carved and sculptured by reason of its being  
“ soft,”—“ it is carved and sculptured,—state passed to fit it for  
“ being so,—softness.”

THE *material*. Δι' ἄλφειτου πεποίημενος, “ made of flour,”—  
“ made,—intervening state passed previous to the making,—the  
state of flour.”

THE *procuring or meritorious cause*.

Ἐχω ἃ ἔχω διὰ σέ κ' ἐκ ἄλλων ἐροτῶν. SOPHOCLE.

“ What I possess, I possess by your merit, (or by your favour),  
“ and by that of no other mortal,”—“ what I possess, I possess,  
“ —medium of its reaching or passing to my possession,—your  
“ merit or favour.”

THE *final*, (nearly related to the preceding) cause. Δι' Ἀθη-  
ναῖες μαχήσεται, “ he will fight on account of the Athenians,”—  
“ he

“ he will fight,—medium to bring about his fighting,—the prospect of benefiting the Athenians.”

’Ου τόσον ἀθανάτων ἀγέμεν σπύδουσι θυηλάς

’Οσον αἰγιρομένων διὰ κάλλεα παρθενικῶν. MUSÆUS.

“ They hasten, not with the view of celebrating religious rites in honour of the gods, but for the sake (or on account) of the beauty of the damsels who assemble on these occasions ;”—they hasten,—medium or passage to induce them to do so,—the beauty of the damsels,—the prospect of meeting the assembled damsels.”

IN several of these applications of διὰ to causation, particularly the last, it must be admitted, that the mode of expression is elliptical and circuitous ; in all of them, however, it is easy to trace, either immediately or remotely, the radical sense of διὰ as a noun, modified by use into a preposition.

#### Eis.

THIS preposition, as well as ἐν, was supposed by Dr MOOR to have been originally the numeral adjective εἷς, μία, ἐν, both prepositions, denoting, as he conceived, *the one space* ; that is, the space immediately adjoining the object spoken of. This idea, though ingenious, appears clogged with insurmountable difficulties. Eis and ἐν are by no means so nearly the same in meaning as this derivation would make them. LINNÆUS derives εἷς from the verb ἵω, *to send*,—a derivation undoubtedly forced and unnatural ; and SCHEIDE makes εἷς a contraction of εἶσω, *within*, evidently reversing the analogical order of the language, εἷς being in fact the parent of εἶσω.

NONE of these deductions being at all satisfactory, for explaining the various uses of εἷς, we must have recourse to some other origin of the word ; and one better adapted to the purpose  
may,

may, I think, be found. There is in common use a defective verb ἡμαι, signifying *to sit*. From the analogy of the language, it is clear that this must have been the perfect passive of an obsolete verb ἔω, signifying *to set*, of which the perfect passive would consequently signify, “I have been set,” or “I am set,” or what is equivalent, “*I sit*.” Now, in the regular declension of this verb ἔω, εἰς would occur as the participle of one of the aorists passive, naturally and literally therefore signifying *set down*, or what is equivalent to this, any thing in the act of sitting, resting, or some similar attitude. Here, as in the other prepositions, some common word being understood, εἰς might come directly and literally to express *sitting place, resting place, stopping place*.

WITH this sense, all the uses of εἰς exactly coincide.

Ἡ εἰς τὰς παραλλήλους εὐθείας εὐθεῖα ἐμπίπτουσα, (EUCLID), “the straight line falling on parallel straight lines,”—“the straight line falling,—resting place,—parallel straight lines.” Ἐπεξεγράφη ἡ κανὼν εἰς τὸν ἄλιον, (ARCHIM.), “let the ruler be turned towards the sun,”—“let the ruler be turned,—resting or stopping point of its turning,—the line of the sun.

—— ἦλυθον εἰς Τροίην.

HOMER.

“I came to Troy,”—“I came,—stopping place,—Troy.”

Εἰς τοὺς Ἕλληνας χρυσὸν ἐκόμισε, “he caused money be sent to the Greeks,”—“he caused money be sent,—stopping place,—place where the sending rested,—the Greeks. Ἐβάλλε λίθον εἰς τεῖχος, “he threw a stone at or against the wall,”—“he threw a stone,—stopping place,—the wall.” Εἰς τὴν ἐκκλησίαν καθέζομαι, “I set myself down in the assembly,”—“I set myself down,—sitting place,—the assembly.” Κυρὸς παρὲν εἰς τὸ αὐτὸ χωρίον, (XENOPH.), “CYRUS was present at, or in the same place,”—“CYRUS was present,—resting place,—the same place.”

Ναῦται

Ναῦται εἰς Ἑλίαν τε καὶ ἀστέρας Οὐρίωνος

Ἐδρακὸν ἐκ νηῶν.

APOLLON. Rod.

“The sailors from the vessels looked towards the Bear and the stars of Orion,”—“the sailors looked,—resting place of their looking,—place where their view rested,—the Bear and Orion.”  
 Εἰς τριακόσιους ἐγένοντο συναμῶνται, “the conspirators amounted to 300,”—“the conspirators proceeded in number,—point where the numeration stopped,—300.” Ἐἰς ἡλίου καταδύντα δαίνονται, “they feast till sunset,”—“they feast,—stopping point of their feasting,—sunset.” Εἰς δύναμιν ἐργάζεται, “he works to the utmost of his power,”—“he works,—stopping point,—his strength,—point where his exertion stops,—the point where his strength stops.” Ἐγκλήματα ἐς τοὺς Ἀθηναίους, “complaints against the Athenians,”—“complaints,—object where they rested,—the Athenians.” Ἐπαινέται εἰς τὸ κάλλος, “he is praised for his beauty,”—“he is praised,—point where the praise rests,—his beauty.”

IN all these various applications of εἰς, we find the same radical idea of *stopping place* or *resting place* exists; and, in like manner, it would be easy to trace the same primitive sense in every phrase or sentence where this preposition occurs.

#### Εκ.

IN the *Etymologicum Parvum* of GREGORY, the derivation of εκ is properly stated from the verb ἐκω, “I yield, give way, or quit my place.” This preposition, therefore, appears to be a mutilated verbal noun, of similar import with the verb, and may therefore be exactly rendered *quitted*, or *place quitted*, and, by a very obvious transposition, *state quitted*. This radical meaning corresponds with that of the English word *out*, which seems a branch

branch from the same root with the French *ôter* or *ôster*, to take away or remove.

UNDER this radical and original sense of *εκ*, all its different significations may be properly arranged.

# I. PLACE.

Ἐκ Πύλου ἔλθων.——

HOMER.

“Coming from Pylos,”—“coming,—place quitted,—Pylos.” Πολὺν ἐκ ποταμοῦ χρυσὸν ἀφύσσασμενος, “drawing much gold from or “out of the river,”—“drawing much gold,—place quitted,—“the river.” Λαῶς ἐξ ἔρειος τμήθεις, “a stone cut out of the “mountain,”—“cut out,—place quitted,—the mountain.”

2. TIME. Ἐκ τῆς ὑπατείας τέθνηκε, “he died after the consulship,”—“time left,—period quitted,—the consulship.” Ἐκ τῆς δεῖπνου ἀνεχώρησεν, “he went away after supper,”—“he went “away,—time quitted,—the time of supper.”

3. STATE. Ἐξ εἰρήνης πολεμεῖν, (THUCYD.), “out of peace to go “to war,”—to go to war,—state quitted,—peace.”

UNDER this idea, *ἐκ* is often applied to the materials from which a thing is made, because the object formed is, by an obvious analogy, considered as having quitted the mass of unformed matter to assume its new state. Ποτήριον ἐκ χρύσου, “a cup of “gold,”—“a cup made,—state quitted,—the mass of unformed “gold.” Ἐκ τριῶν εὐθείων τρίγωνον συστήσασθαι, (EUCLID). “from “three straight lines to construct a triangle,”—“to construct a “triangle,—state or form quitted,—three unconnected straight “lines.” Λίθων ἐξ ὧν τὰ ἀγάλματα ποιῶσι, (THEOPH.), “stones, of “which statues are made,”—“statues are made,—state quitted, “—mass of unformed stone.”

Ἐκ and ἀπὸ are frequently used indiscriminately. From the radical sense of the two, such an idiom is perfectly admissible; for as *ἐκ* signifies *the place quitted*, and *ἀπὸ* *the remote point*, or *point*.



*point of departure*, it is plain, that the idea implied in the one may often be precisely the same with that conveyed by the other.

## Εν.

I HAVE already taken notice of Dr MOOR's idea, that εἰς and ἐν were, both of them, parts of the numeral adjective εἰς, μία, ἐν, an unsatisfactory derivation, by no means conveying the radical sense of either preposition. As we sought the origin of εἰς from another quarter, the same, I apprehend, must be done with regard to ἐν. LINNEP's derivation of ἐν, from the verb εἰμι, *sum*; and SCHEIDE's supposition, that it is a participle from the verb εἰμι, *immitto*, are certainly inadmissible; the former giving no determinate sense to the preposition, and the latter being particularly faulty, as requiring the preposition itself, or one of similar import, to be joined to the verb, before it could acquire the sense there assigned to it.

Εν, then, I think, may be justly deduced, and, indeed, must be actually derived, from the verb εω, or εννύω, *to clothe*, and thence *to cover*. By direct procession from this root, ἐν, whether a verbal substantive, or, as is not improbable, a contracted participle, retains the signification of the verb; and its radical sense may therefore be set down as *clother*, *coverer*, and, by generalizing this idea, *container* or *comprehender*. Let us try how far its various applications may be resolved by this idea.

THE most natural and common application of ἐν, is to PLACE, and that in different modes. Εν χειρὶν ἔχω, "I have it in my hands,"—"I have it,—container,—my hands." Δίθος ἐν τῇ ἄκρᾳ τῇ Ερινεάδι καλεσμένη πολλύς, (THEOPHR.), "abundance of stone in the promontory called Erineas,"—"abundance of stone,—container,—the promontory called Erineas." Παράλληλογράμμα ἐν ταῖς αὐταῖς παραλλήλοις. (EUCLID), "parallelograms between the same parallels,"—"parallelograms,—container,—

“the same parallels.” Πάραλληλόγραμμον συστήσασθαι ἐν τῇ δοθείσῃ γωνίᾳ, (EUCLID), “to construct a parallelogram,—container,—a “given angle,”—“a parallelogram formed with a given angle.” Ἐν τῷ οἴκῳ ἐκαθίζετο, (JOHN xi. 20.), “the fat in the house,”—“container,—the house.” Ἐλαχίστη ἐν τοῖς ἡγεμόσιν Ἰουδα, (MAT. ii. 6.), “least among the chiefs of Judah,”—“least of those,—“container or comprehender,—the chiefs of Judah,—of those “whom the title Chiefs of Judah comprehends.” Κατέβαινεν ἐν τῇ κολυβήθρᾳ, (JOHN v. 4.), “he descended into the pool,”—“he “descended,—container of him after his descent,—the pool.” Ἐλθεῖν ἐν Ἑλλάδι, “to come into Greece,”—“to come,—contain- “er after the coming,—Greece.” In these two last examples, and others of a similar nature, the intermediate step is, as it were, overlooked, being understood as necessarily implied in the completion of the action. In the English preposition *into*, the expression is more full, though the order of the two prepositions of which that word is compounded, is, as it were, inverted,—*to* implying the termination of the motion by which a place is reached, and *in* the position of the object, after having been fixed at its place of destination.

Εν is also applied to TIME; and in this application the same resolution of it may be made. Ἐν ἑπτὰ ἡμέραις ἀποθνησκούσιν, (HIPPOCR.), “they will die within seven days,”—“they will die,—“period comprehending the time of their dying,—seven days.” Ἐν σπονδαῖς ἐποίησε, “he did it during the truce,”—“he did it,—“container of the time of doing it,—the truce.”

Εν is likewise used in expressing MODES OF ACTION. Ἐν πέλταις καὶ ἀκόντιοις μαχόνται, (XENOPH.), “they fight” (as usually rendered) “with shields and darts,”—“they fight in the way of “shields and darts,”—“they fight,—container of their mode of “fighting,—the use of shields and darts.” Ἀποκτεῖναι ἐν ῥομφαίᾳ καὶ ἐν λιμῷ, (AROC. vi. 8.), “to kill in the way of sword and fa-  
“mine,”—

“mine,”—“to kill,—container of the mode of killing,—the operations of sword and famine.”

LASTLY, *ἐν* is sometimes employed, when RELATION is signified. *Ἐν ἐμοὶ ἐστὶ*, “it is in my power,—within my reach,”—“it is,—container or comprehender,—me,—container of this object,—the line of my agency.”

By a particular mode of phraseology, the Greeks sometimes appear to have made use of *ἐν*, where we might have supposed *ἐκ* or *ἀπὸ* should have been employed. *Ἐν δένδρῳ κόπτειν κλάδον*, “to cut a branch from the tree,”—“to cut a branch,—container of the place where it was cut,—the tree.” In our mode of expression, *from* would have been used, as denoting the point of departure. In the Greek phraseology, it was sufficient to mention the place where the action took effect.

THE Greek preposition *ἐν*, it may just be remarked in passing, appears evidently the primitive of the Latin preposition *in*, and, through it, of the same preposition in most of the modern European languages. But, from some of the preceding examples, it will also appear, that the primitive word was made use of in a more extended sense than is to be found in its various descendants.

#### *Επί.*

THIS preposition is one of those which Dr MOOR has brought forward as a principal illustration of his ingenious theory of the variations of the force of the Greek prepositions, according to the different cases of their connected nouns. Some particular examples, it is certain, seem to support this theory, but others as strongly militate against it; so that I must regard it as by no means sufficiently established: but whether true or not, does not much concern the subject of the present inquiry; for, even admitting the truth of this theory, it goes very little way in assisting us to develop the radical sense of the prepositions.

In the preposition we are now considering, though affording one of the best illustrations of Dr Moor's ideas, the primitive sense must be sought for, not according to his hypothesis, but by a fair examination of its root and cognates.

THE root of ἐπί appears to lie in the verb ἐπω, a verb occurring in different passages of the earlier Greek writers, in its simple form, and of pretty general use in a compound state. 'Επω is rendered by STEPHENS *to work, to handle, to follow*; sometimes *to hurt*, and sometimes *to warm*; and frequently this verb is used by HOMER to express *rubbing, furbishing, and scouring*. All these different senses are, I think, easily reducible to the idea of *pressing or close touching*, which may therefore be taken as the radical sense of ἐπω. 'Επω, the root, having this radical meaning, its cognate and derivative ἐπί, must express the same idea; and as ἐπί appears to be a mutilated verbal adjective, with its substantive understood, it is properly and literally *object pressed, adhered to*, or *object touched closely*; and this signification it retains whether applied to *place, time, or relation*.

1. As applied to *place*.

Ἡ σφαῖρα κυλίνδεται ἐπὶ τῆς τραπέζης, "the ball rolls upon the table,"—"the ball rolls,—object pressed by it, or adhered to in so doing,—the table." Ὁ λίθος πίπτει ἐπὶ τὴν γῆν, "the stone falls upon the ground,"—"the stone falls,—object pressed or adhered to by its fall,—the ground."

Ὀλέσω δὲ πολέας ἐπὶ νηυσὶν Ἀχαιῶν.

HOMER.

"to destroy many of the Greeks at or beside the ships,"—"to destroy many,—place or objects close touched in doing so,—the ships." Καθίζεσθαι ἐπὶ τὴν ἐστίαν, "to sit down at or beside the hearth,"—"to sit down,—place touched, or closely adhered to in sitting,—the hearth." Εὐθῆα ἐπ' εὐθῆαν σταθεῖσα, EUCLID); "a straight line standing upon a straight line,"—"a straight line

“line standing,—place or object adhered to, pressed, or close touched in its standing,—another straight line.” Ἐπ’ Αἴγυπτον ῥέειν \*, (THUCYD.), “to flow towards Egypt,”—“to flow,—place adhered to, pressed, or touched in its flowing,—Egypt.”  
2. As applied to *time*.

Τίς πρῶτος; τίς δὲ ἐπὶ πρῶτῳ; EURIP.

“who first, and who next after the first?”—“who first,—and who adhering to, pressing, or close-touching the first,”—“just after the first?”

Ὅχνη ἐπ’ ὄχνη γηράσκει. HOMER.

“pear grows old after pear,”—“pear grows old,—event close-touched in point of time,—the growing old of another pear.”

—— ὅσον ὀλκὰς ἐπὶ τρίτον ἡμᾶς ἀνύσση. DION. GEOG.

“As much as a ship of burden would pass in three days,”—“would pass,—period adhered to, touched or reached in this passage,—the third day.”

3. As applied to *relation, possession or occupation*.

Τῶν ὄντων τὰ μὲν ἐστὶν ἐφ’ ἡμῖν, τὰ δὲ οὐκ ἐφ’ ἡμῖν, (EPICT.), “Of things some are in our power, others not,”—“some are so constituted, that the object adhered to, pressed, or close-touched by them, is the line of our power, or the line of our reach.” Ἐπὶ τοῖς γεγενημένοις χαλεπῶς φέρειν, “to be displeased in consequence of what has taken place,”—“to be displeased,—point  
“ or

\* THIS example is directly against Dr Moor’s hypothesis, that ἐπὶ with the genitive denotes *motion upon*, and with the accusative, *motion directed upon*; here it has the latter signification with the *genitive*. The same thing holds in many other instances, in the best and most accurate Greek writers.

“or object which the displeasure touches, or adheres to,—the things that have happened.” ‘Ο ἐπὶ τῶν δημοσίων λόγων, “the officer over the public accounts,”—“the person so occupied, —that the public accounts are the object touched or adhered to in his occupation \*.”

Οἵτινες ὕμνος ἐπὶ μὲν θαλίαις,  
 ἐπὶ τ' ἀλαπίναις, καὶ παρὰ δαίνοισι,  
 εὖροντο.

EURIPID.

“Who invented songs as an accompaniment at festivals, at convivial entertainments, and at feastings,”—“songs,—object or event which they are made to adhere to, touch, or closely accompany,—festivals and entertainments.”

IT would be tedious to go through the multifarious quotations brought together by STEPHENS and VIGERUS, as various meanings of ἐπὶ in the Greek writers; all of them may with little difficulty be resolved, either in a direct or secondary sense, into the idea of being *pressed, adhered to, touched closely, handled, or some equivalent circumstance.*

κατὰ.

IN tracing the origin of κατὰ, a difficulty occurs from the want of a radical verb, corresponding properly in signification with the sense of this preposition. The difficulty, however, is removed, when we take into view the system of cognates, as evidently found to exist in the Greek language †. From the facts

\* PERHAPS, in this and similar phrases, there is a reference to the secondary sense of ἔπω, “to handle or work upon,—hence to manage;—the same seems to hold in such expressions as ὁ ἐπὶ τῆς πόλεως, “the governor of the city,”—“he who is so placed,—that the object handled or managed by him is the city.”

† SOME observations on this subject have been thrown into an Appendix, being too long for a note.

facts established by this system, it is ascertained, that in these primitive roots, one signification belonged to verbs where the radical consonant was the same, though the vowels were varied. That thus, for instance, the roots *καω* and *κew* originally denoted the same idea, though, in the progress of the language, the traces of one of the roots might be lost. Now the verb *κew* must evidently have been the present indicative active, whence the defective verb in common use, *Κᾶμαι*, "I lie," originally was formed, this last being the perfect passive, with the augment omitted. Upon this analogy, *κew*, and of course its cognate *καω*, signified to *lay*, or *lay down*; and *κατὰ*, a verbal noun, proceeding from this root, had originally the literal meaning of *laying place*, *lying place*, or place where an object was lying or laid down. This sense we shall find it still retains, in its different applications, in the form of a preposition.

Εὐθεῖα τέτμηται κατὰ τὸ Δ σημεῖον, (EUCLID), "the line is cut at the point D,"—"the line is cut,—laying place, or place where the cut lies,—the point D." Συμπίπτουσιν κατὰ τὸ Κ, (EUCLID), "let them coincide at K,"—"let them coincide,—point where the coincidence lies,—K."

Κατὰ ραπὴν πυκνὰ κείμεθα. HOM. Od.

"We lie,—laying place, or place where we were laid,—the thick bushes,"—"among the thick bushes."

Τὰ μὲν δασόμεθα κατὰ σφίσι. APOLLON.

"These things we will divide among them,"—"these things we will divide,—place where the division lies,—them." Κατὰ βορρῆαν ἑστηκώς, (THUCYD.), "standing or situate toward the north,"—"—situate,—point where the situation lies,—the north."

THIS sense of *κατὰ* corresponds precisely with what is usually attributed to it when joined with the accusative, viz. *apposition*; and

and, if justly analyzed, the same will be found to hold when it is joined with the genitive, though, with that case, *κατὰ* is commonly said to express *opposition*. Thus, *κατὰ σκοπῆ τοξεύειν*, (HERODIAN), “to shoot at a mark,”—“to shoot,—lie of the shot,—or place where the shot was made to lie,—the mark.” In those cases where *κατὰ* falls to be translated as denoting *opposition*, the opposition unquestionably lies in the connected verb, not in the preposition.

Κατὰ γῆς πίπτειν.

DION. HALICAR.

“To fall against the ground,”—“to fall,—lie or direction of the fall,—place where the falling lay,—the ground.” But in most instances where *κατὰ* occurs with the genitive, no direct opposition is intimated. Thus,

Κατ’ ὀφθαλμῶν κέχυτο ἀχλὺς.

HOMER.

“A mist was spread, (not against, but) *before* his eyes,”—“a mist was spread,—place where its operation lay,—his eyes.”

FROM this primary sense of *κατὰ*, two secondary senses appear to have branched. In the first place, any thing laid down evidently occupies a certain space, in the same position or direction with the object upon which it lies. Upon this idea, *κατὰ* came frequently to be used to express *the line along which any thing has its position, course, or direction*. Hence it may frequently be rendered *along*. *Εἶσι κατ’ ἔρειος*, “he goes along the mountain,”—“he goes,—direction of his going,—lie of his course,—the mountain.” *Ἐρχονται κατὰ τῆς ὁδοῦ*, or *κατὰ τὴν ὁδόν*, “they come along the road,”—“they come,—direction or lie of their coming,—the road.” *Κατὰ τῆ τειχέος κρέμασαι*, “to suspend along the wall,”—“to suspend,—direction in which the object was suspended,—the wall.” By the obvious analogy between place and time, *κατὰ* in this mode of application was sometimes



sometimes transferred to express *during*, as it were, *along a particular period*.

Κατὰ νύκτα περιπλάνημαι.

ANACR.

"I wander during the night,"—"I wander along the period of night." Sometimes, too, by an extension of this signification to *modes of relation*, κατὰ came to be used to denote *according to*, that is, in the same direction with another. Κατ' εἰκόνα τῆ κτίσαντος, (Coloss. iii. 10.), "according to the image of the maker,—formed,—direction of the form,—the image of the maker." The Latin preposition corresponding to this was *secundum*, evidently the gerund of *sequor*, and signifying *following*. The analogy in the formation of the prepositions in the two languages was pretty similar.

AGAIN, another secondary sense which came to be affixed to κατὰ, was *down*. This application of the word arose, either, as Dr MOOR supposes, because *down* being the natural direction which bodies take when left to themselves, the word which denoted direction simply, was applied to signify this natural tendency; or, what is fully as probable, because an object *lying* or *laid down*, occupies the lowest position of which it is susceptible. In whichever of the two ways it happened, it is certain, that a very common use of κατὰ is to signify *down*. "Ὁρμησε κατὰ τῆ κρημνῆς, "he rushed down the steep,"—"he rushed down,—place where the movement lay,—the steep,—direction,—that which bodies take when left to themselves,"—"or that which puts them in the lowest place they can occupy.

κατὰ δὲ σφί κελαιὸν;  
Αἷμ' ἀπέλαβειτ' ἔραζ'.

HESIOD.

"The black blood flowed down them to the ground,"—"the blood flowed,—place where the movement lay,—them,—direction,—

“rection,—that which bodies take when left to themselves,—  
 “or that which puts a body in the lowest place it can occupy.”

### Μετὰ.

THIS is one of the Greek prepositions which assume significations apparently so remote, that, at first view, it is not easy to reduce them to one radical sense, however confidently we may rest upon the principle, that there must have been such a radical sense, from which the different significations branched out. *Μετὰ*, it is well known, is commonly used in three different meanings; with the genitive denoting *with*, with the dative *among*, with the accusative *after*. The analogy between the two former is sufficiently obvious; but the coincidence of either of them with the third seems at first unaccountable. Upon examining the word, however, by the principles of etymology, perhaps this apparent incongruity may be accounted for.

*Μετὰ* appears to be an immediate derivative from the obsolete verb *μεω*, *I go*; a verb in common use in Latin, though in great measure disused in Greek. *Μετὰ* being thus an immediate descendant from a verb signifying *to go* or *travel*, its primary signification most probably was *a way-post*, *a way-director*; a sense nearly the same with what it still retains in Latin, *meta*, *a goal*. From this sense of way-post or way-director, *μετὰ* would soon be transferred to express *a conductor of the way*, or *a guide*, of whatever kind this might be. Our own word *guide* seems to have been formed by a similar analogy. It is *guida* in Italian, and *guia* in Spanish; which last clearly demonstrates the origin of the whole to have been the Latin word *via*, “*a way*.”

EXPLAINING *μετὰ*, then, by *guide* or *conductor*, its different uses may be easily resolved. With the genitive it denotes a conductor or guide who accompanies us, or whom we accompany;  
 hence

hence it is usually translated *with*. With the accusative it denotes a guide or conductor, who goes before us, or whom we follow; hence it is usually rendered *after* \*. With the dative it denotes a plurality of conductors, and that we are between or amidst them; hence it commonly stands for *among*. Thus the three significations of *with*, *among*, and *after*, take their rise; agreeably to which we may analyze a few of the cases in which *μετά* occurs, and these will sufficiently prove the justice of the explanation.

I. Μετά, with the *genitive*, commonly denoting *with*. Thus, μετά Πλάτωνος ἀδικεῖν, “to do injustice with PLATO,”—“to do injustice,—conductor PLATO.”

——— χαίται ἑρπύοντο μετά πνοῆς. HOMER.

“Their manes were shaken with the wind,”—“their manes were shaken,—conductor the wind.” Μεθ’ ἡμῶν ποιήσασθαι τὸν ἀγῶνα, “to engage in the contest with us, or on our side,”—“to engage in the contest,—conductor in the contest,—us.” Ὁ ποίησας τὸ ἔλεος μετ’ αὐτοῦ, (*Luk. x. 37.*), “he that shewed mercy on him,”—“that shewed mercy,—guide or direction of the channel in which the mercy was to flow,—him.”

It is to be observed, that, between a guide or conductor whom we accompany, and a mere companion in the way, the difference is very often hardly perceptible. It was very natural, therefore, in their mode of applying *μετά*, to slide from the one to the other. Hence *μετά* with the *genitive*, in a variety of instances, denotes little more than a mere conjunction, or accompaniment, the strict idea of guiding or directing being thrown

Y y 2 into

\* AFTER, though the common, is not the universal sense of *μετά* with the accusative; sometimes, with this case, it signifies the same as with the dative, Βάκτρον ἢ αὐτῷ μετά χειρὸς, (HERODIAN), “he had a staff *between* his hands.”

into the shade. Σώφρονες μετ' ἀνδρείας, καὶ ἀνδρείοι μετὰ σώφροσυνης,  
 "temperate with fortitude, and brave with temperance,"—  
 "temperate,—companion, fortitude,—and brave,—companion,  
 "temperance." Τὸ ΑΕΚ τρίγωνον μετὰ τῷ ΚΖΓ τρίγωνῳ, (EUCLID.),  
 "the triangle ΑΕΚ, with the triangle ΚΖΓ,"—"the triangle  
 "ΑΕΚ companion or accompanier the triangle ΚΖΓ."

Μετὰ ἑαίρειαν αἰῶδαν. ANACR.

"Singing with the lyre,"—"finging,—accompaniment,—the  
 "lyre."

2. Μετὰ with the *dative*, signifying usually *among* or *between*.

Μετὰ πρωτοῖσι πονήτο. HOMER.

"He was busy among the foremost,"—"he was busy,—conduc-  
 "tors or companions furrounding,—the foremost."

Οὔτιν' ἐγὼ πύματον ἔδομαι μετὰ οἷς ἐταροῖσιν. HOMER.

"I will devour Noman the last among his associates,"—"I  
 "will devour Noman last,—conductors or companions in de-  
 "struction,—his associates." Τὸν μὲν μετὰ χερσὶν ἐρυσάτο, "he  
 "caught him between the hands,"—"he caught him,—guides  
 "in catching him, or objects on either side of,—the hands."  
 Δεινὸν δ' ἐς Δανῆν μετὰ κύμασι, "it is a terrible thing to die by  
 "or among the waves,"—"to die,—conductors to death, or ob-  
 "jects furrounding in death,—the waves."

3. Μετὰ with the *accusative*, usually rendered *after*.

Πρόσθε μὲν ἵππῆες, μετὰ δὲ νέφος εἵπετοπιζών. HOM. II.

"first the horse,—and after them followed a cloud of foot-sol-  
 "diers,"—"first the horse,—and a cloud of foot-soldiers follow-  
 "ed,—conductors or guides,—them." Οἱ δὲ μετὰ τῆτες ὀκτώ,  
 (ARCHIMED.), "the eight after them,"—"the eight coming,—  
 "conductor preceding them,—them." Μετὰ τὸν πόλεμον ἦκειν,  
 (PLATO),

(PLATO), "to come after the war,"—"to come,—leader or predecessor in point of time,—the war." Μετὰ τὴν θλίψιν τῶν ἡμερῶν ἐκείνων ὁ ἥλιος σκοτιθήσεται, (LUK. xxiv. 29.), "after the tribulation of these days the sun shall be darkened,"—"the sun shall be darkened,—conductor preceding this event,—the tribulation of these days."

Οἱοί καὶ Δάναοισιν ἀριστῆες μετέασι καὶ μετ' Ἀχιλλῆα. HOM.

"What chiefs there are among the Greeks even after ACHILLES,"—"what chiefs there are,—persons attending them, or accompanying them,—the Greeks,—even conductor preceding them,—ACHILLES."

As any object we are in pursuit of, and seek to obtain, may with great propriety be said to be the conductor that we follow, μετὰ with the accusative is often used to express such an intention or action.

\*Ὅς με μετ' ἀπρήκτους ἔριδας καὶ νείκεα βάλλει. HOM. II.

"Who drives me in pursuit of vain quarrels and contests,"—"who drives me,—object to which I am driven,—or which I am made to follow,—vain quarrels and contests."

———— τὴν ποτὲ Νήλεως,  
Γάμεν εἶν μετὰ κάλλος.

"Whom NELEUS married on account of her beauty,"—"whom NELEUS married,—object of his pursuit in doing so,—or object by which he was led,—her beauty."

Ζεὺς γὰρ ἐπ' ὠκεανὸν μετ' ἀμύμονας Αἰθιοπῆας,  
Σθιζὸς ἔβη μετὰ δαῖτα. HOM. II.

"For JUPITER was gone the day before towards the ocean, among the virtuous Ethiopians, to hold a festival,"—"JUPITER,"

“TER was gone,—conductors in respect to place,—the Ethiopians,—conductor in respect of object in going,—to hold a festival.”

ΜΕΤÀ having often the sense of *after*, frequently transfers that sense to the words with which it is joined in composition. It thus, in many cases, comes to express a change having taken place, an event different from the one that preceded it. Νόέω, “I consider.” Μετανοέω, “after one mode of thinking I go to another,—I change my mode of thinking,—I repent.” Τίθημι, “I place.” Μετατίθημι, “after having placed the objects in one mode, I pass to another mode of placing them,—I transpose.” When μετὰ in composition denotes participation, this is nothing more than a transferring of its other signification *among* or *with*. Ἐχω, “I have,” μέτεχω, “I have along with,—I partake.”

WHETHER alone or in composition, therefore, μετὰ appears to be always a substantive noun, denoting guide, conductor, or leader in the way.

### Παρά.

IT seems to be allowed by the most accurate Greek grammarians, that the radical meaning of παρά, in one modification or other, is *besides*. This, I think, brings us pretty near the primitive idea which it was used to express. LINNÆUS derives it from the adjective παρὸς, *before*; and SCHEIDE, both παρά and παρὸς, from the verb πάω, *to press*. Both these, however, appear rather wide of the proper sense of the preposition. Παρά, in fact, appears to be an old substantive noun, denoting precisely *side* or *flank*. It must be admitted, that no direct examples of παρά, in this form of a noun, are to be found, at least I have not been able to meet with any such; but the sense of the word, when it occurs as a preposition, whether by itself or in composition, sufficiently indicates its original form; and though, as a noun, it does

does not itself occur, yet in some of its immediate derivatives, traces of the same signification remain. Thus, in Greek, we have *παρα*, a *cheek*, and in Latin we find the adjective *par*, "equal," both evidently descendants from this root; the former denoting the side parts of the head, the latter an object corresponding side for side with another. As, therefore, the English preposition *beside*, is nothing but a contracted form of *being side*, that is, being the side of an object; so the Greek preposition *παρά*, beside, may justly be set down as a noun signifying *side* or *flank*; and from the different aspects under which an object may be viewed, as occupying the side of another, the different applications of *παρά* take their rise.

IN the mathematical writers this is plain. *Παρά τὴν δοθεῖσαν εὐθεῖαν παραλληλόγραμμον παραβαλῶν*, (EUCLID), "to construct a parallelogram upon (as commonly rendered) a given straight line,"—"to construct a parallelogram,—side of it,—a given straight line." And in composition, *παραπληρώματα παραλληλογράμμου*, "the complements of a parallelogram," as commonly rendered, mean exactly the *side fillers*.

IN writers of a different class the same meaning appears. *Κελόιος παρά κελόιον ἰζάνει*, (ARISTOT.), "daw fits beside daw,"—"daw fits,—side,—daw."

———— *πὰρὰ κροτάφων τὲ παρειάς.* HOMER.

"The cheeks beside the temples,"—"the cheeks,—side,—the temples."

———— *αγορὴν ἢ παρὰ νῆυσι τέτυκτο.* HOM. *Od.*

"The market-place which was formed beside the ships,"—"which was formed,—side or flank of the market-place,—object by which the market-place was flanked,—the ships."

Βῆ δ' αἰκεῖν παρὰ Δῖνα πολυφλοίσβοιο θαλάσσης. HOM. II.

"He went in silence beside the shore of the far resounding ocean,"—"he went,—side of his course, the far resounding ocean."

FROM this radical meaning of *παρὰ*, the transition was short and easy to apply it to express a particular *side or party*, when any opposition was spoken of. *ἵεναι παρὰ τῷ Τισσαφέρνηϊ*, (XENOPH.), "to go over to TISSAPHERNES,"—"to go,—side to which he goes,—that of TISSAPHERNES." *Ἐσί κ' παρ' ἐμοί τις ἐμπειρία*, (DEMOST.), "there is also some experience on my side,"—"there is some experience also,—side me."

ANOTHER transition, perhaps equally easy, was to apply *παρὰ* in expressing some *particular view* taken of an object. Here, as before, the idea is derived from *side*; the subject being supposed to have several sides on which it may be viewed, and the one spoken of the particular one immediately in view; as,

Θεός ἔδοκ' ἐν παρὰ τὸ μέγεθος τῶν πεπραγμένων. LUCIAN.

"I seemed a god for the greatness of my actions,"—"I seemed a god,—side on which I was viewed,—the greatness of my actions."

It was natural, too, to apply *παρὰ*, when *contrast or comparison* in any manner was to be denoted; the two objects contrasted being considered as placed side by side, for the purpose of more accurate observation. *Παρὰ τὰ στυφρὰ ὁ οἶνος γλυκύτερος*, (ARISTOT.), "wine is sweeter contrasted with bitter things,"—"wine is sweeter, bitter things being set by its side."

Ἐχει τὴν ὀγκόν Ἀργος Ἑλληνῶν παρὰ. EURIP. *Phon.*

"Argos has some ground of boasting beside the Grecians,"—"object set by its side,—the other Grecians." *Ὠγαῖος ἀλλεὶ παρὰ*



παρὰ τῆς οὐχίας τῶν ἀνθρώπων, (*Psalms* xlv. 2.), "fair among the children of men;"—"fair,—objects placed beside,—the children of "men."

WE find *παρὰ* also applied in two different modes, which at first sight might be deemed opposite, but, rightly explained, are found to accord. It is used sometimes to express that a thing falls short of, sometimes that it goes beyond, the object with which it is connected. Τεσσαράκοντα παρὰ μίαν, (2 *Cor.* xi. 24.), "forty bating one,"—"as far as one on this side of forty \*." Παρὰ τὸν ποταμὸν ἔφυγον, "they fled beyond or to the farther side of the river,"—"side where their flight terminated,—the river." Παρὰ, in both these cases, radically means *at the side of*, and nothing more; the particular side, whether nearer or more remote, being left to be discovered from the connection of the sentence †.

WITH the genitive, *παρὰ* pretty frequently, though not universally, is made to signify *from at*, or *from beside* an object. Παρὰ βασιλέως πορεύομενον, "coming from beside the king." In such phrases where it is evident a complex sense is affixed to the word, I suspect a compound preposition has originally been in use, viz. *παρὲν*, a preposition still frequently met with, and exactly expressing this idea. By degrees, it is probable the *εν* came to be dropped, the meaning in most cases being sufficiently plain from the connection. Such omissions may be found in every language, and, to those accustomed to the idiom, they occasion no ambiguity.

*Παρὰ* in composition frequently denotes *some deviation from* the exact idea expressed by the connected word. Λογίζομαι, "I Vol. V.—P. II. Z z "reckon;"

\* THIS, I believe, however, is a mode of expression not very common.

† "DUAS significationes contrarias patitur," says VIGERUS, "*supra et infra*," but does not attempt by any explanation to reconcile this apparent contradiction.

“reckon;” *παράλογίζομαι*, “*I reckon false, I misreckon.*” The idea here is plain: the primitive word is supposed to mark a direct course, and *παρά*, signifying *side*, marks, that instead of holding this direct course, the person goes to a side.

WE find, then, *παρά*, in all its different applications, retaining its original meaning of a substantive noun, denoting *side*, in some one particular point of view.

### Περί.

THIS is a preposition of more uniform and simple application than the preceding. It is evidently an immediate cognate of *πέρας*, a *boundary*, and I should conjecture it to be the dative of this very noun, contracted from *πέρατι* and *πέραι* to *περί*. Its precise meaning, therefore, is *circumference*, a word equivalent to *entire boundary*. Hence, it is easy to see, how *ἀμφὶ* and *περί* come to have nearly the same signification; the former expresses *the object contained*; the latter, its correlative, *the containing boundary*. *Περί*, therefore, may be rendered, in general, *circumference* or *boundary*, or *boundary*.

It is thus that the sense of it is to be resolved.

————— *περὶ σπᾶούς.* HOMER.

“Round the den,”—“bounding or furrounding—the den.”

————— *ἔνδον περὶ στήθεσιν χιτῶνα.* HOM. *Od.*

“He put a tunic round his breast,”—“he put a tunic, forming the boundary of his breast.”

————— *περὶ ῥοδείοισιν ἕρση τήκεται.* APOLLON. *Rhod.*

“The dew distils round the roses,”—“the dew distils,—a circumference to the roses,”—“forming a circumference to the roses.” *Τῶν περὶ τὴν διάμετρον παραλληλογράμμων*, (EUCLID),  
“of

“of parallelograms round the diameter,”—“parallelograms,—boundary of the diameter,—forming the boundary of the diameter.” Περὶ τρίγωνόν κύκλον περιγράφαι, (EUCLID), “to describe a circle round a triangle,”—“to describe a circle,—a circumference or boundary of the triangle.”

THOUGH ἀμφὶ and περὶ, as commonly used, are almost synonymous, yet we sometimes find the poets, to make their expression more copious, join both prepositions with the same noun.

— Ἡμεῖς δ' ἀμφὶ περὶ κρήνην ἱερὰς κατὰ βωμούς,

ἔρδομεν.

HOMER.

“We were sacrificing round about the fountain and the sacred altars,”—“we were sacrificing,—place comprehended by us,—the circumference of the fountain and the sacred altars.”

Περὶ, signifying circumference or boundary, is sometimes applied to denote *near*; the surrounding object, in this case, being supposed to go about the other without precisely touching it. Ἐπίπτε περὶ τὸ δένδρον, “he fell near the tree,”—“he fell,—in the space bounding or boundary of the tree.”

It is chiefly when applied to time that this last sense of περὶ occurs. Περὶ μεσημβρίαν ἦλθε, “he came about noon,”—“he came,—time in which his coming happened,—the period surrounding noon,—not actually at it.”

It was by an easy and obvious transition from the radical sense, that περὶ was made to express *concerning* or *about* a subject, meaning the subject which the thought or the discourse comprehends or forms the boundary of. Γογγυσμός πολὺς περὶ αὐτοῦ ἦν, (Jo. vii. 12.), “they murmured much concerning him,”—“they murmured,—the murmur comprehending him.” Περὶ τῇ πόλει δεδιέναι, (THUCYD.), “to be afraid for or concerning the city,”—“to be afraid,—object which the fear included or comprehended,—the whole city.”

Περί sometimes, both simply and in composition, denotes *superiority*, plainly from the idea, that the object which forms a boundary or circumference to another, must of course exceed it in bulk or greatness.

————— Ἐστὶ περὶ νόον ἑρῳτῶν. LUCIAN.

“It is beyond the comprehension of mortals,”—“it is such that it is capable of going round, and consequently exceeds the comprehension of mortals.”

It was observed, in the remarks upon ἀμφὶ, that in Latin *circa* and *circum* expressed the idea of *round* or *round about*. These are both nouns, precisely similar in their force and meaning to περί.

### Πρὸ.

Πρὸ is supposed by LINNÆUS to be the same with πρὸς, and by SCHEIDE to be a part of the adjective πάρος, *before*. The former appears evidently erroneous, the two prepositions, though so nearly related in sound, being in fact very different, as we shall find, both in origin and meaning. The latter idea, however, that πρὸ is a part of πάρος contracted, seems extremely probable. The adjective πάρος, signifying properly *fore*, is still in use. In speech this word, like many others, might suffer an abbreviation of the first syllable, and be pronounced πρὸς instead of πάρος. From this contracted adjective we have the comparative πρότερος, *prior*, and the superlative πρότατος, contracted into πρῶτος, *foremost* or *first*. Of this adjective πρὸς, the preposition πρὸ appears to be the dative, signifying, therefore, with a substantive understood as in other instances, the *fore part*, *fore object*, or *object in front*; and as it was always followed by the genitive case of the succeeding noun, and the genitive case in Greek has usually the same force with the particle *of* in English, viz. *species* or *kind*,

kind, the Greek preposition *πρὸ* denotes precisely *fore object in respect to*.

Thus we find, in respect to *place*. 'Ἔστηκε πρὸ τῆς πυλῶνος, "he stood before the door,"—"he stood,—front object in respect to him,—the door."

In respect to *time*. 'Εγένετο πρὸ τῆς πολέμου, "it happened before the war,"—"it happened,—being a fore event in respect to the war."

In respect to *preference*. 'Αρετὴ πρὸς χεήματων, "virtue before riches,"—"virtue, an object in selection preceding riches."

In respect to *defence or protection*. Πρὸ τῶν παίδων ἢ τῶν γυναικῶν μάχεσθαι, "to fight in defence of wives and children,"—"to fight,—being the front object in respect to your wives and children."

IN transferring this original word *πᾶρος* from the Greek, the Latins have been so accurate as to form two prepositions from the primitive adjective; *præ*, derived from the feminine *πρῆ*, and retaining the literal meaning of *fore* or *before*; and *pro*, derived from the masculine or neuter, used chiefly in the secondary, but more extended sense, answering to the English *for*, *on account of*.

I MAY add here, that our English preposition *for*, appears to have a similar origin with the Greek *πρὸ*. In the "Ἐπεὶ Πτεροεῖντα, indeed, a different source is assigned to the English *for*; it is said to be an abbreviation of the Saxon *farina*, a *cause*. Had Lord MONBODDO or Mr HARRIS ascribed to it such a genealogy, how would HORNE TOOKE and Dr BEDDOES have ridiculed the fancy of originating a preposition from the abstract and metaphysical idea of causation. In fact, this derivation is far-fetched, and it evidently required no small straining to twist some of the applications of *for* into the line of cause and effect\*. Without going so far, a plain and obvious origin

\* Thus,—“This may do *for* once, but not *for* ever,”—"cause once,—or cause ever," would give hardly any sense. "It is good *for* nothing,"—what meaning.

origin of it may be found in the word *fore*; and upon examination I think it will be found, that *for* in English, and  $\pi\epsilon\delta$  in Greek, and *pro* in Latin, as well as the German *vor* or *für*, means, in its radical sense, *position before*; with this difference only, that in Latin, English, and German, the word denoting the fore object generally follows the preposition, but in Greek precedes it. In English, therefore, the preposition *for* may be always taken as meaning simply *object*. Substitute *object* in all the phrases in which the author of the *Diversions of Purley* has put *cause* as the meaning of *for*, and it is easy to observe how naturally and exactly the sentences may be resolved.

### $\Pi\epsilon\delta$ .

$\Pi\epsilon\delta$  is evidently a preposition of very extensive application, and in investigating its original meaning, a variety of opinions have prevailed. Dr MOOR resolved it into  $\pi\epsilon\delta$  and  $\acute{\alpha}\varsigma$ , and supposed that it denoted properly *in the fore space of*. SCHEIDE ascribes to it a meaning somewhat similar, deducing it from  $\pi\acute{\alpha}\gamma\epsilon\varsigma$ , *before*. Both these derivations, however, are unsatisfactory. In some of the modes in which  $\pi\epsilon\delta$  is used, the idea of *before* may indeed be traced; but, in others, it cannot, I think, without the most violent straining, be discovered. It will be necessary, therefore, to look out for a different origin, and one carrying with it an idea more consonant to the signification and uses of the preposition. This, I think, can only be found in the  
noun

ing could here be assigned to—"cause, nothing?" "He is tall *for* his age,"—"cause, his age," would denote something quite different from what is intended. "For a good harvest, a good summer is necessary,"—"cause, a good harvest," would completely invert the meaning, making the effect the cause. "He lived there *for* twenty years,"—"cause, twenty years," would be unintelligible. In these instances, taking *for* to mean *object placed before*, or *object in view*, the sense is obvious. The same may be said of such phrases as the following: "He is a good man *for* ought I know,"—"he is a good man,—*object*, or put in objection, "ought I know," an easy and plain resolution; whereas, "cause, ought I know," would be extremely forced, if not altogether unmeaning.

noun *πρας*, a *termination*, *limit*, or *terminating boundary*. This noun in the genitive forms *περάτος*, which, according to the common analogy and practice of the language, would be gradually contracted into *περάος* and *περῶς*, and lastly abbreviated into *πεῶς* or *περὸς*. If this deduction be correct, (and I think it bears the marks of truth), the radical sense of the preposition *προς* must be exactly *termination*, or *terminating in*. From this idea all its different senses may naturally be traced.

IN the mathematical writers, we find *προς* retaining its primitive meaning. 'Αἱ *προς τῇ βάσει γωνίαι*, (EUCLID, L. I. p. 4.), "the angles at the base,"—"the angles,—termination,—the base,—terminating in the base." *Τὴν πλευρὰν προς ταῖς ἴσαις γωνίαις*, (EUCLID, Lib. I. pr. 26.), "the side *betwixt* the equal angles," (as commonly rendered), "the side,—termination,—the equal angles,—the side whose terminations are the equal angles." *Προς τὰς τῶν κύκλων περιφερείας εὐθεῖα προς ἐλήθωσιν*, (THEOD. Lib. 2.), "let straight lines be drawn to the periphery of the circle,"—"let straight lines be drawn,—termination,—the periphery of the circles."

THE application of *προς* to other sensible objects, is resolvable in a somewhat similar manner.

—— *προς μὲν ἄλως Κᾶρες*. HOMER.

"The Carians at the sea,"—"the Carians,—boundary the sea, whose country is bounded by it."

—— *ποτὶ πόλιος πέτετ' αἰά*. HOMER.

"He still continued flying towards the city,"—"he continued flying,—termination of his flight,—the city."

'Αἱ μὲν *προς βορέαο καταβαιοὶ ἀνθρώποισιν*. HOMER.

"Those to the north are passable for men,"—"those,—termination  
"tion

"tion the north,"—or "boundary,—the north,"—are passable  
"for men."

———— νῆας ποτὶ σπιλάδεσσιν ἔαξαν,  
Κύματα. HOMER.

"The waves dashed the ships against the rocks,"—"dashed the  
"ships,—termination or terminating point of the dashing,—the  
"rocks." *Πρὸς κέντρα λακτίζειν*, (*Acts ix. 5.*), "to kick against  
"the spikes,"—"to kick,—termination of the action,—the  
"spikes."

FROM sensible objects, *πρὸς* was transferred to objects of dif-  
ferent descriptions, where the original idea denoted by it was  
meant to be expressed or implied. *Οὐ πρὸς ὑμῶν ἔστι*, (*Isoc.*), "it  
"does not belong to you,"—"it is not a thing whose termina-  
"tion is you." *Οὐ νομίζουσιν τὴν ἀρετὴν πρὸς τῷ σφετέρῳ ἀγαθῷ πεφο-  
ρίναι*, (*XENOPH.*), "they do not reckon virtue for their good,"  
"—they do not reckon virtue as terminating in their good."

*Πρὸς Διὸς εἰσὶν ἅπαντες.* HOMER.

"All are from Jove,"—as commonly rendered,—"*all refer to*  
"*Jove as the centre in which they terminate.*" *Πρὸς ἀνδρῶν ἔχων*  
*τὴν συγγενείαν καὶ οὐ πρὸς γυναικῶν*, (*DEMOST.*), "having relationship  
"on the male, not the female side,"—"having his relationship,  
"—termination or boundary,—point to which it is to be tra-  
"ced back,—the male, not the female side." *Πρὸς τοῖς εἰρημένοις*,  
"in addition to what has been said,"—"boundary or termina-  
"ting point to which the succeeding remark reaches,—what  
"has been said."

———— *πρὸς αἰσχέουσιν ἄλγεα πάσχει.* HESIOD.

"He endures afflictions along with disgraces,"—"he endures  
"afflictions,



“afflictions,—bounding point,—point to which they reach,—disgraces.”

We find *πρὸς* frequently applied to denote that one object is affected, regulated or governed by another; because, in the regulating object, we find the boundary which limits and defines the idea in view. Ἐὰν πρῶτον πρὸς δεύτερον τὸν αὐτὸν ἔχῃ λόγον, (EUCLID), “if the first have the same ratio to the second,”—“if the first have the same ratio,—defining boundary of the ratio, —the second.” Ποιήσας πρὸς τὸ θέλημα, (Luke xii. 47.), “having acted according to his will,”—“having acted,—boundary or limits within which the action was confined,—his will.”

In this manner may *πρὸς*, varied and anomalous as it appears, be traced through all its applications to one determinate idea.

In support of the opinion now stated, that *boundary, limit, or termination*, was in fact the radical sense of this preposition, it may be added, that among the most ancient Greek writers, the poets, the word *ποτὶ* was used as synonymous with *πρὸς*. This is commonly said to be according to the Doric dialect; but so great a change of the word does not, I think, fall within the usual rules of formation of that or any other dialect of the language. The real cause of the interchange I suspect to have been, that *ποτὶ* and *πρὸς*, though two quite different words, came to have the same meaning affixed to them in the following manner. *Πρὸς*, as we have seen, is a contracted case of *πέρας*, a termination or boundary; *ποτὶ*, again, seems to be just the Doric dative of *πῆς*, “the foot,” for *πόδι*, agreeably to the common analogy of that dialect. That the word signifying the extremity of the human body should be extended in use to denote a termination in general, was perfectly natural, as it can hardly be doubted, that in many instances the names of the parts of the human body were transferred by analogy to express the different forms of position in sensible objects of every description.

## Σύν.

Σύν, in regard to its application, is one of the simplest of the Greek prepositions; its radical and primitive meaning being in every case easily perceptible. Its origin appears to be in the verb *σύνω*, to *sew*, or to *join together*,—a verb which though obsolete and out of use in Greek, being supplanted by the synonymous word *ῥάπτω*, has, however, been retained unchanged in Latin, a circumstance which warrants the inference, that in the earlier stages of the Greek language, when the Æolic, the immediate parent of the Latin, was the common dialect, *σύνω*, in the sense of *joining*, was in general use. Of this verb *σύνω*, *συν* seems to have been a contracted participle passive; and thus in its radical meaning denotes *joined*, *object* or *circumstance joined*, the analogy being the same with what has been shewn in the "Ἐπεὶ Πτερόεντα, to take place in regard to the English preposition *with*, a part of the verb *withan* to join.

From this radical sense of *σύν* all its various uses naturally take their rise, all of them implying junction in place, time, or acting. Ἐρχόμεθα ἡμᾶς σύν σοί, (*Jobn* xxi. 3.), "we come with you,"—"we come,—object joined,—you." Τὸ ὑπὸ τῆς ὅλης σύν τῇ προσκειμένη, (*EUCLID*), "the square described by the whole line, together with the part added,"—"the whole line, —object joined thereto,—the added part." Σύν πάσι τέτοις ἄλλο κακὸν ἐγένετο, "together with all these another misfortune happened,"—"another misfortune happened,—circumstance joined,—all these before enumerated." Σύν τῷ δεπνεῖν ἀπηλθε, "he went away in time of supper,"—"he went away,—event joined in time to his going away,—supper."

Σύν ᾧ καὶ Ὑλας κίεν. APOLLON.

"With whom went *HYLAS*."—"HYLAS went,—person joined  
"him."

"him." Σὺν σοὶ μάχοιμην, "I would fight with your aid,"—"I would fight,—person joined to me in fighting,—you." "Ὡς οἱ εἶη σὺν Θεῷ εἰρημένον, (HERODOT.), "that it was spoken to "him by divine authority,"—"that it was spoken,—circumstance joined thereto,—divine authority."

## Ὑπέρ.

THE only two remaining prepositions in Greek are ὑπέρ and ὑπὸ. These are evidently derived from the same root, and in their radical sense denote the same idea, though, by a different mode of applying it, their common acceptation has the appearance of being nearly opposite to each other. The original of both was the adjective ὕψος, signifying *high*. Of this adjective, though fallen into disuse, there are many immediate derivatives and cognates to be found: ὕψος, *high* or *sublime*, *height* or *elevation*; ὑψηλός, *lofty*; and directly arising from the primitive word, the comparative ὕπερος, contracted from ὕποτερος, *higher*, and the superlative ὑπάτος, contracted from ὑποτατος, *highest* or *supreme*. From the comparative ὕπερος, comes the preposition ὑπέρ, radically denoting a *higher object*, hence commonly used to express *placed over* another. The analogy of the English preposition was similar; *over* being the comparative of the adjective *op* or *ob*, aspirated into *ov*, whence by a regular formation came the comparative *over*.

Ὑπέρ, then, appears to have originally denoted, that one object was *higher* in respect of, or in comparison with another with which it was connected; and, in this case, the noun to which the radical force of the preposition refers, is the one that *precedes* it in the sentence, differing in this from most of the other prepositions, and in particular, as we shall immediately see, from its cognate ὑπὸ, which generally refers to the one that comes *after*.

Taking *ὑπέρ*, then, as denoting simply *higher in respect of*, we shall find all its different uses satisfactorily explained.

1. *Over or above, in place :*

Στῆ δ' *ὑπέρ* κεφαλῆς.

HOMER.

"He stood over his head,"—"he stood,—higher in respect of his head,—higher than his head." *Ἵπέρ τῶν κήπων ἕρως κᾶται*, (HERODOT.), "the keeper lies above the gardens,"—"the keeper lies,—higher in respect of the gardens."

2. *Superior to, in point of operation.* *Τὰ ὑπέρ ἡμᾶς εἰδέν πρός ἡμᾶς*, (*Proverb*), "the things beyond or above us are nothing to us,"—"the things,—higher in regard to their effects than us, are nothing to us."

— *ὑπέρ* μοῖραν.

HOMER.

"Superior to fate,—controlling fate,"—"higher in respect to operation than fate."

3. *Beyond, from the usual analogy, by which the speaker considers himself as the centre of observation, and consequently expresses himself as if the more remote a place was from him it became as it were higher in the circle in respect to him.* *Αἰθιοπίας τῆς ὑπέρ Αἰγυπτου*, (THUCYD.), "Æthiopia which is beyond Egypt,"—"Æthiopia, which is farther from the centre of observation, consequently higher in the circle, in respect to Egypt."

4. *In defence of*, because a thing which defends another covers it from assault, and what covers is naturally accounted higher. *Ἵπέρ σε μάχομαι*, "I fight in defence of you,"—"I fight to cover you, consequently in fighting am higher than you."

5. *In room of.* In this application of *ὑπέρ*, I suspect there is an ellipsis of some such word as *τόπος*, *place*; *ὑπέρ* here implying as it

it were *over the place from which another is removed*, the primitive idea of *higher* being still, though more remotely, retained. Ὑπέρ σὺ διακονᾷ, "he serves in room of thee,"—"he serves,—coming "over the place from which you have been removed,—or which "you have left unoccupied."

6. *Concerning*, by a similar analogy to what takes place with us when we say, *to think over*, *to talk over* a subject, the thought or discourse being supposed, as it were, to cover the subject thus thought or spoken of. Ὅσα ὑπέρ τῆς εἰρήνης κατεψεύσατο μὲν, (DEMOST.), "what he falsely charged me concerning the peace,"—"what he charged,—subject of the charge covering or going "over the peace."

Thus, in all the different applications of ὑπέρ, we find the radical idea denoted by it is *higher*, in some cases more directly, in others extended by analogy to express some particular view in which one object either is made to be, or is considered as being higher than another.

### ὑπὸ.

IN analyzing ὑπέρ, it was found to be the comparative from the adjective ὕψος. Of this adjective the preposition ὑπὸ appears to be the dative, with some common word denoting place or position understood. In the application, however, of ὑπὸ and ὑπέρ as prepositions, the signification of the two, though originating from the same root, was reversed; in the one case, the attribute being referred to the noun preceding; in the other, to its correlative, the noun following. In these, therefore, there is no contradiction, but merely a diversity in the mode of application. *High* and *low*, *above* and *under*, are merely relative terms. When one object is *low* or *under* in reference to another, this last, of consequence, comes to be *high* or *above* in reference to the first. Such a position of two objects, therefore, may be equally expressed by saying, that the first is above the second, or  
the

the second is under the first. In the preposition *ὑπέρ*, the Greeks used the one mode of expression; in the preposition *ὑπό* the other. In the English prepositions by which these are rendered, a similar analogy in both cases takes place.

As the adjective *ὑψος*, therefore, originally and literally signified *high*, the preposition *ὑπό* has radically the same meaning, and to this primary idea its various uses have a direct reference.

I. LITERALLY and most usually, it denotes *under* or *below* in point of *place*, from the analogy already explained. Καὶ τὸν ὑπὲρ γῆς καὶ ὑπὸ γῆν χρυσόν, “the gold both above and under the ground,”—“the gold,—higher in respect of the ground, and “the gold in regard to which the ground is higher.”

“Ἄμα κάτεκοντο, ὁ μὲν ἄνω, ὁ δ’ ὑπ’ αὐτόν. LUCIAN.

“They sat down together, the one above, the other below him, “(ZENO),”—“they sat down together, in such a position, that “the one was above ZENO,—and ZENO was high—in regard to “the other.”

“Ἄλλο μὲν ἐξεσάωσεν ὑπ’ ἰλύος. APOLLON.

“He saved one fandal from under the mud,” “he saved one “fandal,—place quitted,—the situation in which the mud was “high in regard to it.”

“Ἐνδον ἔμιμνε πίθε ὑπὸ χείλεσιν. HESIOD.

“It remained within, under the edges of the vessel,”—“it remained within, in such a situation that the edges of the vessel “were high, or in a high position in regard to it.”

By an easy extension of this meaning, *ὑπό* came to denote that one object was in any way covered or hid by another. Καὶ μιν κατακρύπτει ὑπὸ τὴν θύραν, (HERODOT.), “he conceals him behind “the door,”—“he conceals him in such a position that the door “covers

“covers him from view,—consequently, in that sense, may be said to be above or high in regard to him.”

2. It is applied in some particular cases to denote a portion of time. The idea seems to have been expressed elliptically in such cases, something more than bare duration being implied; an intimation being also given, that the portion of time so marked out was distinguished by some property conducive to the action.

——— ἦλθε ὑπὸ νυκτι.

APOLLON.

“He came *under* night,” *i. e.* “under cover of night.” Ἀπὲξ ὃς ὑπὸ σκότι, “he went off under darkness,”—“under cover of darkness.” Here the primitive idea of ὑπὸ is still discernible, though more remotely marked; the properties which distinguish night or darkness being reckoned the cover or protector of the coming or going off. By a similar analogy arises our English expression *under* night.

3. It sometimes expresses the relation of *superiority* and *subjection*. The connection with the primitive idea is here perfectly obvious. Αἴγυπτος δὲ πάλιν ὑπὸ βασιλείας ἔγενετο, “Egypt came again under the government of a king,”—“Egypt came to be again in the situation in which a king was the high or governing power in the state.” Ὑπὸ Πέρσῃσιν ἐστὶ Αἴγυπτος, (HEROD.), “Egypt is under the dominion of the Persians,”—“Egypt is in such a situation that the Persians are high in regard to it.” Ἐχων ὑπ’ ἐμαυτὸν στρατιώτας, (Matth. viii. 9.), “having soldiers under me,”—“having soldiers in such a state that I am high in regard to them.”

4. Ὑπο is not unfrequently applied to denote the *agent*, *instrument*, or *directing cause* of an action. This application probably arose from the idea, that as in the agent, instrument or directing cause, there was plainly implied some sort of power or dominion

over

over the action, these fell, therefore, to be viewed as the superior or higher objects in regard to it.

Ἐξείδε χθὼν πᾶσα, καὶ οὐρανὸς ἠδὲ θάλασσα,  
Ῥιπῇ ὑπ' αἰθανάτων. HESIOD.

“All the earth, and heaven, and sea, were put in commotion under the force of the onset of the immortals,”—“the onset being the acting or presiding cause,—hence the higher power in regard to the effect.”

Κάματο τεθνηῶτες ὑπὸ βλοσυροῖσι λέεσι. HESIOD.

“They lay dead by means of,—or under the force of,—furious lions,”—“the lions being the cause or instruments of the death, the power whence the death originated,—hence the higher power in relation to it.” Ὑπὸ τῶν δικαστῶν ἐκπεσεῖν, “to go into banishment by the sentence of the judges,”—“to go into banishment,—the judges being the presiding power,—the superior power in regard to that effect.” Ἀποθανεῖν ὑπὸ τῶν πολεμίων, “to fall by means of the enemy,”—“the enemy being the superior power in relation to that event.”

IN all its various applications, then, we find ὑπὸ a noun, retaining its primitive force; in some cases, it must be owned more remotely, but in all sufficiently plain to enable us to trace it by the principles of just grammatical theory.

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SUCH, then, are the various prepositions in Greek, analyzed according to what I conceive the true principles of philosophical grammar. In the course of the investigation, it may be observed, no great weight has been laid on the particular cases of the



the nouns with which the different prepositions are joined, and which, in technical language, they are said to govern. In laying down the grammatical principles of the language, this is undoubtedly a matter of some importance; but in investigating the origin and radical sense of the prepositions, little light, I apprehend, can be derived from that circumstance. Even in regard to their signification and use, it is far from being a certain or accurate guide. Dr MOOR has indeed constructed an ingenious theory about the mode of their application, deduced from the cases they are found to govern; but the principles of this theory are in many cases contradicted by the uses of the prepositions in the most accurate Greek writers \*. And, indeed, I suspect the theory

\* Dr MOOR supposes, for instance, that the following distinction in the use of the prepositions, with the different cases, may be regarded as constant and certain. With the *genitive*, he thinks, that the peculiar relation denoted by the preposition is represented as in a state of rest or continued junction with the object: with the *accusative*, as in a state of tendency towards: with the *dative*, as in a state of junction under some particular modification. Thus, he says, *ἐπὶ* with the genitive denotes rest or situation upon; with the accusative, motion directed upon; with the dative, some particular mode in which one object may be said to be upon another. He applies the same theory to *ὑπὸ*, and some of the other prepositions governing different cases. But many examples might be brought in direct opposition to this theory. Thus, in THUCYDIDES, *ἐπὶ Θρακῆς στρατεῖαν πάρεσκευάζειναι*, “he prepares an expedition,—directed upon or against Thrace.” *Ἐπὶ Αἰγυπτῶν ῥέειν*, “to flow towards Egypt.” In HOMER we find,

——— οὕτω μοι εὐκλείη εἴη ἐπ’ ἀνθρώπων.

“So may my fame be established or exist among men,—resting upon or among men.” In like manner, we find *ὑπὸ* with the genitive denoting tendency under.

Ἰάδης ὑπὸ Πριάμοιο.

QUINT. SMYRN.

“He comes towards the presence,—or under the view of PRIAM.” The same preposition occurs with the accusative very frequently, denoting rest or situation under. *Κατακρύπτει ὑπὸ τὴν θύραν*, (HEROD.), “he conceals it, resting under or behind the door.”

——— κατακρύπτει

ory itself is too refined and philosophical to be admitted as just in the use of a language modified by general use, and adapted to the common converse of men. I have, therefore, all along, rather declined resting on such a ground, conceiving it in general to be uncertain and fallacious. I wished, in tracing the real sense and origin of the prepositions, to proceed, if possible, by a different, and what appeared to me a more satisfactory route. How far the track I have chosen has enabled me to reach the object in view, must be left to the judgment of those best acquainted with the principles of the language.

## APPENDIX.

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κατακείμενο ὑπὸ τοῦ αὐτοῦ.

LUCIAN.

“They sat down,—the one resting or situate under him.” These examples, and many more might be produced, seem to prove the fallacy of the ingenious Professor’s theory, and show that the Greeks were by no means so philosophically accurate in the use of their prepositions as he supposed.

## APPENDIX.



**I**N the outset of the Inquiry, it was observed, that the Greek language is a language of regular structure, forming its roots within itself. Following out this idea, I have, in different parts of the investigation, had recourse to the line of cognates and derivatives, as the surest aids for conducting us to the radical sense of a word, where the immediate root appeared to have fallen into disuse, or the signification had diverged considerably from the primitive idea. The grounds upon which I have thus proceeded, may require elucidation.

THE regular system of Greek analogy upon which I have rested so much, though actually, I am persuaded, founded in nature and in truth, has been rejected by many, and even treated with ridicule by some, no mean proficient in the language. The fanciful notions which a few zealous enthusiasts for the perfection of the Greek tongue have engrafted on the general theory, and the imperfect manner in which even its more temperate advocates seem to have understood or explained it, have drawn upon the system a degree of discredit, which, if fairly examined, it will hardly, I think, be found to merit. HEMSTERHUIS and his scholars, who appear to have first developed it, were deficient as well in tracing this analogy through the minuter ramifications

of the language, as in pointing out its real foundation in the natural progress of the human intellect, and the gradual formation of the signs of thought. They contented themselves with merely giving the general outlines, and taking the facts as they found them occur in the actual state of the language. Lord MONBODDO, credulous in all things, and anxious to raise our admiration of the Greeks, maintained the strange conceit, that the Greek language was entirely a language of art, invented by men of profound science, who, in order to construct it, first formed certain words as the roots from which the whole was to branch out ; and then, by certain nicely regulated adjuncts of letters or syllables, fabricated new words themselves, and enabled others to form them as they were wanted. Hence, said he, a complete and regular system of analogy could not fail to take place, every part of the language being so exactly adapted by these men of science to the whole and to every other part. Unfortunately, his Lordship neglected either to inform us where and when those scientific architects of the language appeared, or to point out the rules of their procedure in this regular fabrication of words : he went no farther than to state the supposed primitive radicals they had formed, leaving to future inquirers to find out, as they best could, their subsequent ramifications. The ingenious author of the appendix to DAWES' *Miscellanea Critica*, appears to have had a juster idea of the manner in which such an inquiry ought to be conducted. In that appendix, he has given a general statement of the Greek radicals, announcing, at the same time, a farther design of an entire work, on the origin, progress, and connection of ideas, as discoverable in the formation and structure of the Greek tongue. A few specimens of this projected analysis are likewise subjoined. This promised work, it is to be regretted, has not yet appeared. There cannot be a doubt that it will contain many curious and valuable observations ; though, from the specimens given in the above appendix, I must own, that

that ingenious as many of the observations undoubtedly are, there seems reason to question whether the analysis may not, after all, be found too vague and general to lead to a satisfactory result.

THOUGH I am sensible of these deficiencies in the manner in which the different writers I have mentioned have exhibited the system of Greek analogy, I cannot but think, at the same time, that the system itself, if fairly delineated, is by no means an arbitrary creature of fancy, but has a real and solid foundation in the nature and progress of the human intellect. Connected as this subject is with many parts of the foregoing disquisitions, I hope it will not be going too far out of my way to subjoin to this paper some remarks in support of that opinion, together with a few cursory hints, both of the grounds on which I conceive this system of analogy to rest, and the mode in which it might probably be followed out with advantage, to elucidate the real nature and progress of the language. Such a detail will form the best explanation of my reasons for trusting to this guide so much as I have done, in the progress of these inquiries into the radical sense of the prepositions.

IN entering upon this discussion, we may without hesitation adopt in part the Hemsterhusian system of Greek analogy, as detailed by LINNÆUS, and partially caught by Lord MONBODDO; a system which may be regarded as an approximation to the truth, though requiring much both of correction and addition to render it complete.

ACCORDING to this system, the radical primitives in the Greek language are the five duads *αω*, *εω*, *ιω*, *οω*, and *υω*;—all of them verbs, and all signifying the same thing, viz. the general idea of motion, “*I move*,” “*I make to move*,” or “*I cease motion*,” by the addition of consonants to these duads, the signification is modified and defined in any manner that may be required for expressing the various ideas and desires of the mind. Hence  
every

every Greek word may be traced to a verb as its immediate primitive, and all verbs ultimately found to emanate from one or other of these duads, by the regular addition or insertion of the consonants.

ONE obvious defect in this system is, that it leaves unexplained altogether the mode in which the various consonants are used for modifying the general idea denoted by the duads. Hence, were we even to admit the hypothesis as true, by far the greater part of the mechanism of language would still remain to be investigated.

ANOTHER essential defect seems to be, that this system supposes, contrary to all probability, that the abstract idea of motion unmodified was the root of all language, consequently of all the conceptions of the intellect; and that it was not till this idea was formed, that the various kinds and modes of it came to be known and named. The progress of nature seems to be the reverse: individual acts are first observed and named, and from these individual acts all general notions come to be formed.

THE system is farther erroneous in this, that it supposes these five duads to be significant and radical words; whereas it is evident, that the *ω* in each can be nothing but the termination of the verb; and these terminations of the verb, nothing else than the remains of a pronoun, coalesced with the verb with which it had been formerly connected.

IN so far then, I apprehend, we shall find this system of analogy defective and erroneous; yet, at the same time, it conducts us a certain length upon the right road, and is therefore by no means undeserving of attention.

THE natural theory upon which I am inclined to think that a just system of analogy, not for the Greek language alone, but for every primitive language whatever, might be formed, is the following: Language having been formed and employed for the communication of our feelings, whether of pleasure or pain, ob-  
jects

jects were therefore named, (in whatever manner the language originated), from their qualities, as discovered in their actings or effects in regard to us. Hence it follows, that the verb is the root of all language. The sounds by which these feelings were conveyed, were the shortest possible, and would therefore be all monosyllables, consisting probably of one consonant and one vowel sound only. As the vowel sounds are often uncertain and variable in their enunciation, not only in different languages, but in different dialects of the same language, they must therefore have been frequently interchanged in common speech, and no variation in these sounds could be supposed to affect the expression of the particular idea meant to be conveyed. It was to the consonant sounds alone, in which the diversity was fixed and obvious, that recourse was necessarily had, as the radical and uniform signs by which the diversity of ideas or feelings was to be expressed. Each consonant, therefore, had a particular signification, which it always retained, with whichever of the vowel sounds it was united; the vowel sounds serving only as the necessary means of enunciating the consonants; and it being consequently a matter of indifference which of the vowel sounds were thus employed.

HERE, then, we may look for the actual roots of language, traced to their remote and primitive origin; simple monosyllabic sounds, containing one consonant only, enunciated indiscriminately by any of the vowel sounds, at the arbitrary option of the speaker.

IN applying this theory to the structure of the Greek language, the Hemisterhusian system of Greek analogy comes opportunely to our assistance, at once supporting and supported by the general observations now thrown out. The duads  $\alpha\omega$ ,  $\epsilon\omega$ ,  $\iota\omega$ ,  $\omicron\omega$ , and  $\upsilon\omega$ , from which, according to this theory, all the words in the language emanate, are in fact, after removing the  $\omega$ , (which, as already mentioned, is an adventitious termination), nothing but

but the vowel sounds serving for the enunciation of the consonants. It is altogether an error, therefore, to state these as of themselves significant : but the framers of the analogical theory were misled by this circumstance, that as the vowel sounds were necessary for the enunciation of the consonants ; as all sounds were meant to express some species of action ; and as all action necessarily implies motion, or the cessation of motion ; it seemed therefore not unnatural, that the necessary component of the significant sound should be supposed to denote the equally necessary component of the action or idea signified.

HAVING ascertained the application of the vowel sounds, the next object is to consider the application of the consonants. In regard to this, it may be observed, that whatever is thought of the hypothesis of the duads, it at least implies nothing absurd or improbable to suppose, that when men began to name objects from their action, as experienced in their effects, the same or similar feelings would naturally be expressed by the same or nearly similar sounds ; that the expressive part of these sounds was the consonant, and that, consequently, when the desire to indicate a feeling or idea once signified recurred anew, this would be done by a repetition of the same consonant sound which had first been employed to make it known. Hence it will follow, that each different consonant, when enunciated, would soon come to denote a particular range of ideas, agreeing among themselves in some common quality, distinct from what were expressed by any of the others. What particular class of ideas each consonant was to denote, was purely arbitrary and accidental. This, therefore, can never be ascertained by theoretical conjecture : if it be known at all, it must be traced by the observation of facts, as they actually occur in the existing primitive languages.

WHETHER, by such a mode of investigation, the actual formation of language could be fully and satisfactorily developed, I cannot pretend to determine. I must own, however, that it  
seems



seems to me not improbable, that by proceeding upon such a system to analyse any primitive language, some curious and interesting facts regarding the formation of speech and the progress of thought might be discovered.

FROM a slight and imperfect survey of the Greek, (which may justly be considered as an original language \*), in its primitive radicals, it appeared to me that something of such a regular analogy was to be found in it; that each of the different consonants, united with the vowel sounds in the form of the duads, was employed to express some one general idea, which might be traced in various ramifications through all the words emanating from that primitive root. The radical ideas, announced by the different consonants, struck me as somewhat of the following nature.

B Impulse, or impulsive force.

Γ Expulsion, extruding force.

Δ Piercing force, or connected with that; dividing force.

Z Expansion or expansive force, force diffusing itself round on every side.

K Laying force, the force by which one object is made to lie upon or occupy a determinate place.

L Abrasion or abrasive force, the force by which objects are stripped, smoothed or pared.

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M Con-

\* I do not here enter at all into the dispute about the origin of the Greek language from the Hebrew, through the medium of the old Pelasgic. In fact, such a derivation does not affect the structure of the language as complete within itself; for this derivation, if real, was not partial, but total: it was not the engrafting of parts upon a language already formed, but a transplantation of the whole, in its native form; so that not only the branches, but the roots, with all their natural ramifications, were carried to, and established in, a new and somewhat different soil. In reasoning, therefore, on the structure of the language, the Greek may justly be reckoned an original language, forming its roots within itself; and the other words from these roots, by a regular progression.

- M Compression, conjoining or compressive force.
- N Cleaving or splitting force.
- Ξ Strong or violent friction.
- Π Adhesion, adhesive force.
- P Fluency, flowing force.
- Σ Tremor, shaking force.
- T Tension, stretching force.
- Φ Eruptive force, force by which interposed obstacles are broken through.
- Χ Opening or disjoining force, force by which two objects once united are made to separate.
- Ψ Smooth or less violent friction.

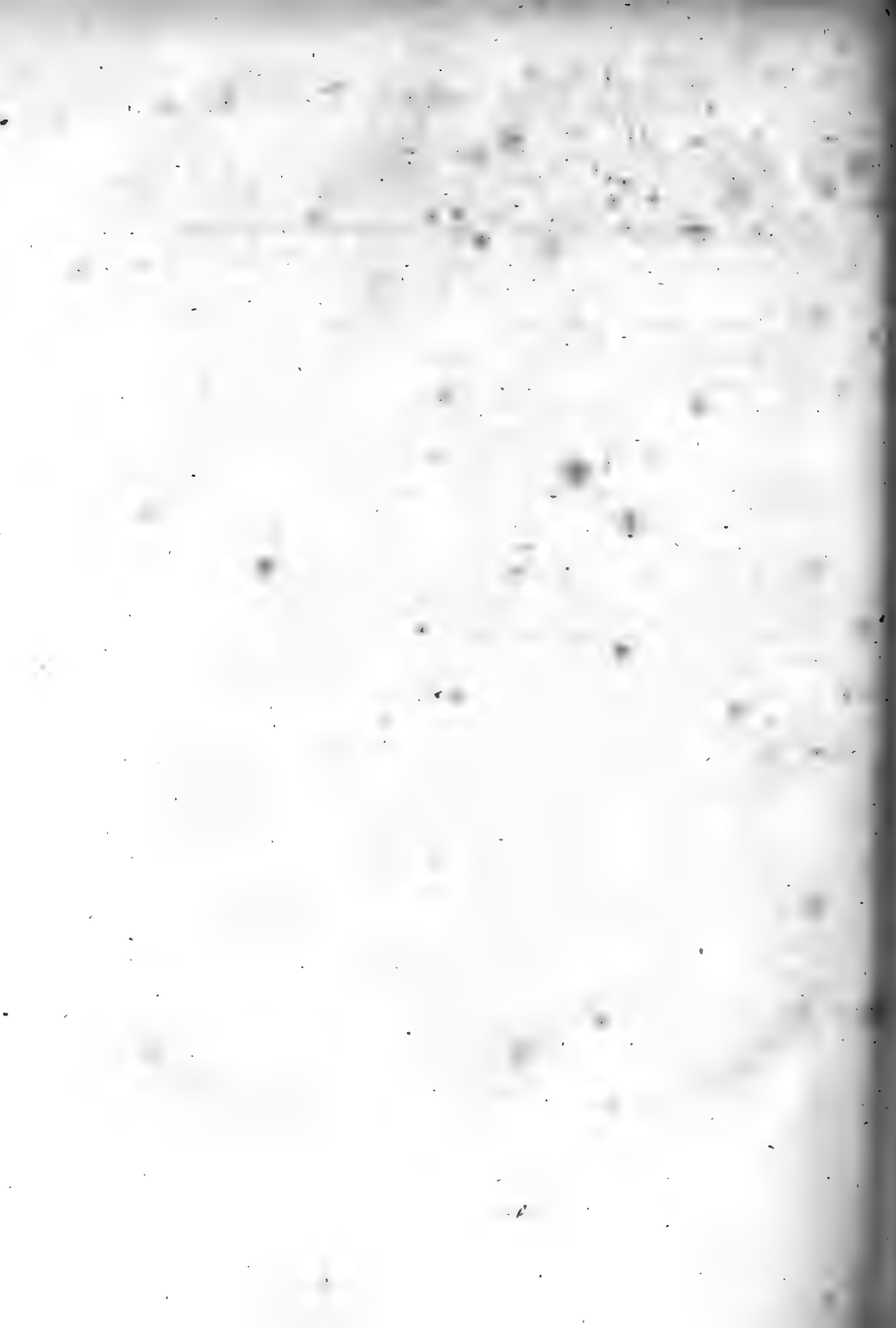
IF it be objected, that some of the ideas thus supposed to be denoted by the radical components of the language, are too abstract and metaphysical to be allowed such a place, I would observe, that in stating these general significations, I by no means suppose the general idea to have been in view when the various words were formed; but that the generalization arose merely from the repeated application of the same sound to express individual objects and individual feelings, concurring in the same common quality; for in fact it is rather the expression than the idea which has a metaphysical aspect, most of the ideas being derived from objects in their nature sufficiently obvious and frequently occurring.

SHOULD it be thought absurd to suppose that all the numerous ideas which speech is intended to express, could ever originate and spread out from the few and scanty sources now laid down, I shall only reply, that if we can trace in fact the whole body of a copious and expressive language, as emanating from these primitive roots, and can find every idea borrowing its expression ultimately from one or other of these radical sounds, we shall have an argument from fact of the truth of the system, which no reasoning from improbabilities can possibly overturn.

AFTER

AFTER all, I by no means wish to be understood as delivering these significations with confidence: they are thrown out as conjectures merely, made upon a cursory investigation, which may probably never be pursued farther. But though these particular conjectures may be erroneous, the system, I am inclined to believe, holds so much of truth, that not only in the Greek, but in every primitive language, if the roots were fully investigated, some common idea, some common force, might be traced out in the radical use of every individual consonant. A theory founded on such an inductive process, would be by no means chimerical. Within the bounds of a particular language, etymology is a pretty safe guide: it is only when rashly applied to various and discordant languages that it is ready to bewilder and mislead.

WERE it possible thus to trace, with any degree of precision, in the roots of different primitive languages, the power and force of the several letters, as employed in their radical words, and from these to follow out the subsequent ramifications of each through all the component parts, an excellent track would thereby be opened for investigating the procedure and advances of the human intellect in the arrangement of ideas and the formation of language. A comparison of these primitive roots would likewise enable us best to determine, whether any, and what, degree of affinity existed among the languages to which they belong. Could the various original languages of the globe be brought into such a point of comparison, the much-agitated question, Whether all are derived from one? might be easily resolved. Probable as the opinion of their common origin must be allowed to be *à priori*, and strengthened as it undoubtedly is by many striking proofs of actual coincidence, still to establish it completely, such an evidence *à posteriori* seems to be wanting, as a radical analysis, founded upon the plan now mentioned, would alone be competent to furnish.



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XIV. EXPERIMENTS *and* OBSERVATIONS *upon the* CONTRACTION  
of WATER by HEAT at LOW TEMPERATURES. By THOMAS  
CHARLES HOPE, M. D. F. R. S. ED. *Professor of Chemistry in*  
*the University of Edinburgh.*

[Read 9th January 1804.]

TO the general law, that bodies are expanded by heat, and contracted by cold, water at the point of congelation, and for some degrees of temperature above it, seems to afford a very singular and curious exception.

THE circumstances of this remarkable anomaly have been for some time believed to be the following:

WHEN heat is applied to water ice cold, or at a temperature not far distant, it causes a diminution in the bulk of the fluid. The water contracts, and continues to contract, with the augmentation of temperature, till it reaches the 40th or 41st degree. Between this point and the 42d or 43d, it suffers scarcely any perceptible change; but when heated beyond the last-mentioned degree, it begins to expand, and increases in volume with every subsequent rise of temperature.

DURING the abstraction of caloric, the peculiarity in the constitution of water equally appears. Warm water, as it cools, shrinks, as other bodies do, till it arrives at the temperature of 43° or 42°. It then suffers a loss of two degrees without any alteration of density. But when farther cooled, it begins to dilate

late, and continues to dilate, as the temperature falls, till congelation actually commences, whether this occurs as soon as the water reaches the  $32^{\circ}$ , or after it has descended any number of degrees below it.

SUPPOSING this peculiarity of water to be established, it must appear, indeed, a very odd circumstance, that heat should produce contraction in this fluid, while it causes expansion in other bodies; and no less strange, that within one range of temperature it should contract, and in another expand, the very same substance. Before a deviation from so general a law should be received as matter of fact, the proofs of its existence ought to be clear and indisputable.

THE experiments hitherto published, from which this singularity has been deduced, have all of them been performed upon water contained in instruments shaped like a thermometer glass, and consisting of a ball with a slender stem; and the expansive or contractive effects of heat and cold have been inferred, from the ascent or descent of the fluid in the stem.

To such experiments it has been objected, that the dimensions and capacity of the instrument undergo so much change, from variation of temperature, that it is difficult, if not impossible, to determine how much of the apparent anomaly ought to be imputed to such changes, and that it is not improbable that the whole of it may be ascribed to them.

THE object of this paper, which I have now the honour to read to the society, is to prove by a set of experiments, conducted in a manner altogether different, that the common opinion is founded in truth, and that water presents itself as that strange and unaccountable anomaly which I have already described.

It is worth while, before detailing my experiments, to give a short account of those observations which led to the discovery of the fact, and which in succession have extended our knowledge of it, as well as of those observations which have at different periods been offered to discredit, and to bring it into doubt.

THE

THE first observation relative to this subject was made by Dr CROUNE, towards the close of the 17th century, while engaged in investigating the phenomena of the great and forcible, though familiar, expansion which happens to water at the instant of freezing; a matter which occupied, in a considerable degree, the attention of his fellow-members of the Royal Society of London, in the earlier years of that institution.

I SHALL relate in his own words his first observation: "I filled  
" a strong bolt-head about half-way up the stem with water, a day  
" or two before the great frost went off, marking the place where  
" the water stood; and placing it in the snow on my leads,  
" while I went to put some salt to the snow, I found it above  
" the mark so soon, that I thought the mark had slipped down,  
" which I presently raised to the water, and as soon as ever I  
" mixed the salt with the snow, the water rose very fast, about  
" one-half inch above it. I took up then the glass, and found  
" the water all fluid still: it was again set down in the salt and  
" snow; but when I came about an hour after to view it, the  
" ball was broke, and the water turned to hard ice, both in the  
" ball and stem \*."

FROM this experiment Dr CROUNE drew the conclusion, that water, when subjected to cold, actually began to expand before it began to freeze. On announcing it, however, to the Royal Society, on the 6th of February 1683, Dr HOOKE immediately expressed strong doubts, and ascribed the ascent of the water in the neck of the vessel to the shrinking of the glass occasioned by the cold.

To obviate this objection, and to preclude, as far as was possible, the influence of the change of capacity in the apparatus from an alteration of its temperature, a bolt-head was immersed in a mixture of salt and snow, and into it, when cooled, was poured, to a certain height, water previously brought to near the  
freezing

\* BIRCH'S *History of the Royal Society*, vol. iv. p. 263.

freezing point. The water began instantly to rise as before, and when it had ascended about one-fourth of an inch in the stem, the vessel was taken out, the whole water remaining fluid.

THESE experiments, supported by others of a similar nature, communicated by Dr SLARE to the Society on the 20th of the same month, appear to have satisfied its members, in general, of this fact, that water, when on the point of congealing, and while still fluid, is actually somewhat dilated previous to the remarkable expansion which accompanies its conversion into ice.

DR HOOKE, however, continued unshaken, and retained the doubts he had expressed.

REMARKABLE as the fact, as now stated, must have appeared, it seems not to have excited particular attention, nor to have solicited more minute examination; and indeed though philosophers did not lose sight of it, yet for near a century no one investigated it more carefully. MAIRAN, in his treatise on ice in 1749, and DU CREST in his dissertation on thermometers in 1757, appear to be well aware of this property of water; but it is to M. DE LUC that we owe the knowledge of the leading and more interesting circumstances, (*vide Recherches, &c.* 1772).

HAVING devoted his attention to the examination and improvement of the thermometer, he was naturally led to the investigation, while engaged in ascertaining the phenomena of the expansion and contraction of different fluids by heat and cold.

HE employed in his experiments thermometer glasses; and the included water, at or near the term of liquefaction, descended in the stem, and appeared to him to suffer a diminution of bulk by every increase of temperature, till it arrived at the 41st degree. From this point its volume increased with its temperature, and it ascended in the tube. This fluid, when heated and allowed to cool, seemed to him to contract in the ordinary way, till its temperature sunk to the 41°, but to expand and increase in volume, as the temperature fell to the freezing point.

THE



THE density of water, he thence inferred, is at its maximum at  $41^{\circ}$ , and decreases with equal certainty whether the temperature is elevated or depressed.

M. DE LUC says, indeed, that very nearly the same alteration in volume is occasioned in water of temperature  $41^{\circ}$ , by a variation of any given number of degrees of temperature, whether they be of increase or of diminution; and consequently that the density of water at temperature  $50^{\circ}$ , and at temperature  $32^{\circ}$ , is the same.

THIS philosopher did not conceive that the constitution of water, in relation to caloric, undergoes a change at the temperature of  $41^{\circ}$ , such that short of this degree caloric should occasion contraction, and beyond it expansion. He imagined that heat in all temperatures tends to produce two but quite opposite effects on this fluid, the one expansion, the other contraction.

IN low temperatures, the contractive effects surpass the expansive, and contraction is the consequence: In temperatures beyond  $41^{\circ}$ , the expansive predominate, and the visible expansion is the excess of the expansive operation over the contractive.

IN 1788, Sir CHARLES BLAGDEN added the curious observations, that water, which by slow and undisturbed refrigeration permits its temperature to fall many degrees below its freezing point, perseveres in expanding gradually as the temperature declines; and that water having some muriate of soda or sea-salt dissolved in it, begins to expand about the same number of degrees above its own term of congelation that the expansion of pure water precedes its freezing, that is, between eight and nine degrees. More lately, (*Philosophical Transactions*, 1801), he, or rather Mr GILPIN by his direction endeavoured to ascertain, by the balance and weighing bottle, the amount of this change of density caused by a few degrees of temperature.

EVERY one must be familiar with the use which Count RUMFORD has made of this peculiarity in the constitution of water,

in explaining many curious appearances that presented themselves in his experiments upon the conducting power of fluids, and in accounting for certain remarkable natural occurrences. The Count, with his usual ingenuity, has endeavoured to point out the important purposes which this peculiarity serves in the economy of nature, and to assign the final cause of so remarkable an exception from a general law.

IN recording the observations and opinions that have been published concerning this point, I might now, in order, notice those of Mr DALTON of Manchester, related in the fifth volume of the *Manchester Memoirs*, which tended to confirm and enlarge our knowledge of it. But as Mr DALTON himself has called in question the accuracy of the conclusions which had been drawn from his experiments, and from those of preceding observers, I shall only remark, that they are of the same nature, and nearly to the same purport, as those of M. DE LUC.

IT was in consequence of a communication with which Mr DALTON favoured me, three months ago, that my attention was directed to this subject. He informed me, that after a long train of experiments he was led to believe that he, and his predecessors in the same field of investigation, had fallen into a mistake with regard to the contraction of water by heat, and its expansion by cold, in consequence of overlooking or underrating the effect which the change in the capacity of the thermometer-shaped apparatus employed, must occasion on the apparent volume of the fluid. He stated, in general terms, that on subjecting water to different degrees of temperature, in instruments made of different materials, he found the point of greatest density was indicated at a different temperature in each.

IN an apparatus, having a ball of earthen-ware, it was at the 34th degree; of glass at the 42d; of brass at the 46th; and of lead at the 50th. And as water could not follow a different law, according to the nature of the substance of the instrument, he  
conceived

conceived that the appearance of anomaly in this fluid originated entirely in the containing vessel, which must cause the fluid in the stem to fall or rise according as its expansions are greater or less than those of the included liquor.

A DETAIL of these important experiments has, ere now, been transmitted for publication in the *Journals of the Royal Institution of London*.

I HAVE already noticed, that Dr HOOKE endeavoured to explain in the same manner the original experiment of Dr CROUNE. This explanation apparently gathers much force from these experiments of Mr DALTON.

It is proper, however, to state, that M. DE LUC was perfectly aware of the alteration in the dimensions of his glass apparatus, but deemed the change too trifling to have any material influence.

Sir CHARLES BLAGDEN paid greater attention to the circumstance, and by calculation attempted to appreciate what allowance ought to be made for the change of capacity in the amount of the apparent changes of volume.

WHEN it is considered, that the whole amount of the apparent change is but very small, and that the expansibility of the glass is with difficulty ascertained, and is variable by reason of the fluctuating proportions of its heterogeneous constituents, it must be acknowledged, that precision in such a calculation cannot possibly be attained, and can scarcely be approached. On this account, all the experiments already noticed are open to the explanation of Dr HOOKE, and in some measure liable to the objection which he had urged. I confess, that the experiments of Mr DALTON, in perfect concurrence with that explanation, created considerable doubts respecting the existence of the peculiarity of water; against the probability of which circumstance, all analogical reasoning, and every argument *à priori*, strongly militate.

UNWILLING to remain in uncertainty, and considering it as a point of much curiosity and interest, I have endeavoured to investigate the subject by experiments conducted in a totally different manner, equally calculated to exhibit the singular truth, but free from the objections to which the others are liable. In them, it was my object to provide, that neither the changes of the actual volume of the water, nor the alterations in the dimensions of the instrument, should have any influence whatever.

I HAVE already taken occasion to state, that the purpose of this paper is to prove, by experiments on the principle now mentioned, that in the constitution of water there really exists the singularity often noticed.

I SHALL first state the plan of the experiments, and then detail the particulars of the most remarkable of them.

WHEN any body is dilated, whether by heat or cold, it necessarily becomes less dense, or specifically lighter; and the opposite effects result from contraction. This is the circumstance, as every one knows, which causes various movements among the particles of fluids, when any inequality of temperature prevails in the mass; hence these particles are little acquainted with a state of rest.

IF a partial application or subtraction of heat produce an inequality of density in a mass of fluid, the lighter parts rise to the surface, or the denser fall to the bottom.

IT readily occurred, that I might avail myself of these movements, and upon statical principles determine the question in dispute.

I HAD only to examine attentively water, as it was heated or cooled in a jar, and to observe, by means of thermometers, what situation the warmer, and what the cooler parts of this fluid affected.

IF I should find that ice-cold water, in acquiring temperature, showed, in its whole progress, the warmer parts near the top, it  
would

would indicate that water follows the usual law, and is expanded like other bodies by heat.

OR if I should observe that warm water, in cooling to the freezing point, had the coldest portion uniformly at the bottom, the same conclusion would follow; while a different inference, and the existence of the supposed anomaly, would be deducible should the event prove different. The only circumstance, I can figure to myself as tending in any measure to render this mode of examining the point doubtful, is, that water near its congealing point may have so little change of density occasioned by a small variation of temperature, that its particles may be prevented by their inertia, or by the tenacity of the circumfluent mass, from assuming that situation which their specific gravity would allot to them.

It will appear, however, very clear, from the circumstances of the experiments which I shall immediately detail, that no obstacle to the success and precision of the experiments proceeded from this source.

It is not necessary for me to relate all the experiments I have made. I shall restrict myself to the detail of six, which present varieties in the modes of procedure, and which afford the most striking results.

#### EXPERIMENT I.

I FILLED a cylindrical jar of glass  $8\frac{1}{4}$  inches deep, and  $4\frac{1}{4}$  in diameter, with water of temperature  $32^{\circ}$ , and placed it on a table, interposing a considerable thickness of matter possessed of little power of conducting heat. I suspended two thermometers in the fluid, nearly in the axis of the jar, one with its ball about half an inch from the bottom, the other at the same distance  
below

below the surface. The jar was freely exposed to the air of the room, the temperature of which was from  $60^{\circ}$  to  $62^{\circ}$ .

THE experiment commenced at noon :

	Top Thermom.	Bottom do.
	$32^{\circ}$	$32^{\circ}$
In 10 minutes,	$33+$	$34+$
— $30^{\circ}$ —	$35.5$	$37$
— $50^{\circ}$ —	$37$	$38+$
— an hour,	$38$	$38+$
— — — and 10 minutes,	$42$	$38.25$
— — — — $30^{\circ}$ —	$44$	$40$
— — — — $50^{\circ}$ —	$46+$	$41+$
— 2 hours and 10 minutes,	$48$	$42.5$
— — — — $30^{\circ}$ —	$50$	$44$
— — — — $50^{\circ}$ —	$50.5$	$45$
— 4 hours,	$54$	$49$

CONFIDING in the indications of the thermometers, from this experiment we learn, that when heat flows on all sides from the ambient air into a column of ice-cold water, the warmer portions of the fluid actually descend, and take possession of the bottom of the vessel.

THIS downward course proclaims an increased density, and testifies that the cold water is contracted by heat. As soon, however, as the fluid at the bottom exhibits a temperature of  $38^{\circ}$ , this course is retarded and soon stopped, and with the rise of temperature beyond  $40^{\circ}$  is totally changed : for when the mass attains this degree, the experiment equally shows, that the warmer fluid ascends and occupies the summit, by its route announcing its diminished density, and proving that water is now expanded by heat.

EXPERIMENT II.

I FILLED the same jar with water of temperature  $53^{\circ}$ ; and that I might observe the phenomena of cooling, I placed it in the axis of a much larger cylindrical vessel, nearly full of water, of temperature  $41^{\circ}$ , and, by an earthen-ware support, raised it about three inches from the bottom, taking care that the water should be on the same level in both vessels. As soon as I had adjusted the two thermometers, as in the former experiment, I observed that the top of the fluid was still at  $53^{\circ}$ , but the bottom had fallen to  $49^{\circ}$ .

	Top.		Bottom.
In 9 minutes,	$52^{\circ}$	-	45
— 15 ———	52	-	44

Now, to accelerate the cooling, I withdrew by a syphon the water from the large cylinder, and supplied its place by ice-cold water, mixed with fragments of ice, which by repeated cautious agitation was kept uniformly at the temperature of  $32^{\circ}$ .

In 23 minutes,	$48^{\circ}$	-	$42\frac{1}{2}$
— 38 ———	44	-	40
— 43 ———	42	-	40
— 46 ———	40	-	40
— 52 ———	36	-	40
— 58 ———	35—	-	39
— 65 ———	34	-	37
— 75 ———	34	-	36
— 103 ———	34	-	34

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THIS experiment is the counterpart of the foregoing, and from the testimony of the same instruments, it appears, that when a cylinder of water of  $53^{\circ}$  is cooled by circumfluent iced fluid, the colder part of the water takes possession of the bottom of the vessel, so as to establish a difference of temperature from the surface, amounting sometimes to  $8^{\circ}$ . And that as soon as the fluid at the bottom arrives at the  $40^{\text{th}}$  degree, the temperature of the fluid in that situation is stationary till the surface reaches the same point.

DURING the subsequent refrigeration, the progress of the cooling undergoes a total change. The thermometers tell that the colder fluid rises to the surface; so that the top gets the start of the bottom soon by  $4^{\circ}$ , and attains the lowest temperature of  $34^{\circ}$  very long before the other falls to the same degree.

THESE circumstances, I think, lead to the conclusion, that by the loss of caloric, water at  $53^{\circ}$  is contracted and rendered specifically heavier, and that this continues to happen till the water come to the temperature of  $40^{\circ}$ , at which period an opposite effect is produced; for now the water, as it cools, becomes specifically lighter, or is expanded.

IN this, as well as the former experiment, the complete change in the situation, which the warmer and colder parts of the fluid affected, in the progress both of the heating and cooling, while every external circumstance of the process continued unaltered, is particularly worthy of remark.

### EXPERIMENT III.

I TOOK a glass jar, 17.8 inches deep, and 4.5 in diameter, internal measure, having a neck and tubulature very near the bottom. I provided also a cylindrical basin of tinned iron, 4.8 inches



inches deep, and 10 inches in diameter, with a circular hole in the middle of the bottom, large enough to receive the top of the jar. By means of a collar and cement I secured this bafon, so that it encircled the upper part of the jar.

THE object of the contrivance was to have the means of applying a cooling medium to the superior portion of a cylinder of water, and it answered the purpose completely. I introduced the ball of a thermometer through the tubulature, till the extremity of it nearly reached the axis at three-fourths of an inch above the rising of the bottom, and having fixed it in this situation, I rendered the aperture water-tight, by a perforated cork and lute.

THIS very tall jar was placed on a table, with the interposition of some folds of thick paper, in a room without a fire, of the temperature  $42^{\circ}$ .

I FILLED it with water of  $50^{\circ}$ , and poured into the bafon, which embraced the top, a mixture of powdered ice and salt.

FROM time to time I explored the temperature near the surface, by inserting the bulb of a thermometer to the depth of half an inch nearly in the axis.

	Bottom.	Top.	Air.
One o'clock,	$50^{\circ}$	$50^{\circ}$	$42^{\circ}$
In 11 minutes,	46.+	—	
— 15 ———	45	48	
— 21 ———	44—	46—	
— 31 ———	42	44	
— 41 ———	41	42	
— 51 ———	40.+	34	{ At this time a thin film of ice began to form in contact with the glass.
— 1 hour 6 min.	40	34	
— — 20 ———	39.5		

	Bottom.	Top.
In 1 hour 44 min,	39.5	
In $4\frac{1}{2}$ hours,	39.5	{ A crust of ice of some thickness now lined the glass, and air had fallen to $40^{\circ}$ .
— $5\frac{1}{2}$ hours,	39	
— 11 hours, <i>i. e.</i> } at midnight, }	39	Crust of ice complete.
— 19 hours, <i>i. e.</i> } next morning, }	39	
— 26 hours,	40.	{ Air $40^{\circ}$ . So much ice had melted that the cake was detached from the side of the vessel, and floated.
— 32 ———	40	
— 41 ———	40	Air $41^{\circ}$ . Ice not all melted.
— 50 ———	41	Air $42^{\circ}$ . Ice not entirely gone.

THIS long protracted experiment presents some striking facts, and its general import, with regard to the subject of investigation, agrees with the preceding. In it we see, that when the frigorific mixture abstracted caloric from the upper extremity of a cylinder of water, nearly 18 inches long, and at  $50^{\circ}$ , the reduction of temperature appeared sooner, and advanced quicker, at its lower extremity than in the axis at the top, not two and a half inches distant from the cooling power. No one can entertain a doubt that this is owing to a current of cooled and condensed fluid descending, and a corresponding one of a warmer temperature ascending. Now, if water observed the same law that other bodies do, and had no peculiarity of constitution, the same progress of cooling should continue. This, however, the experiment teaches us, is not the case: as soon as the fluid at the bottom exhibits a temperature of  $40^{\circ}$ , it ceases. The colder fluid remains at top, and quickly losing temperature, ere long begins to freeze. The continuance of the colder fluid at the surface surely denotes, that it is not more dense than the subjacent warmer

warmer water. The legitimate inference from this is, that water of temperature  $40^{\circ}$  is not contracted by being cooled to  $32^{\circ}$ .

DID water observe the usual law, and lose volume along with temperature, this experiment, by its long duration, afforded ample time for the manifestation of it.

FOR not less than two days did ice-cold water maintain possession of the top, and for the same period the temperature at the bottom never fell below  $39^{\circ}$ . No current, therefore, of cold and condensed fluid moved from the surface, to affect the inferior thermometer, or to attest the contraction of water by cold.

THIS experiment, however, I must remark, does not warrant the conclusion, that the water is actually expanded, though it in no degree opposes it. It proves no more, than that the contraction ceases at  $40^{\circ}$ ; and that water of  $32^{\circ}$  is not more dense than of  $39^{\circ}$  or  $40^{\circ}$ . Nay, some may perchance allege, that it does not prove so much; conceiving, that if at  $40^{\circ}$  the contraction, without ceasing altogether, becomes very inconsiderable, the difference of density occasioned by the subsequent reduction of temperature may be so very trifling, as not to enable the cold particles to take that situation which their gravity assigns to them, in opposition to the inertia and tenacity of the subjacent mass; and therefore that the colder, though heavier fluid, may be constrained to remain above. That this allegation should have no weight attached to it, the circumstances of the succeeding experiment will clearly show, as I shall soon notice.

BEFORE quitting the consideration of the present experiment, it may be worth while to remark, that it may seem rather surprising, that the bottom of the fluid was not apparently affected in its temperature by the ice which so long occupied its surface. It might be expected, though no cold currents descended from above, that the caloric should be conducted from below, and

that the temperature should by that have been reduced\*. I suppose that the caloric did pass from the lower strata upwards, but

\* THIS experiment may perhaps be thought to give countenance to the opinion of the very ingenious Count RUMFORD, that fluids cannot conduct heat, and that no interchange of heat can take place between the particles of bodies in a fluid state, seeing that for two days the fluid at the bottom of the vessel never fell below  $39^{\circ}$ , though the surface was at  $32^{\circ}$ .

FROM the circumstances detailed in his seventh essay, the Count concluded, that heat cannot descend in a fluid. From the present, it might with equal justice be inferred, that heat cannot ascend.

HAD I not the fullest conviction that this celebrated philosopher has pushed his ideas too far, I might be disposed to consider this experiment as according well with the hypothesis.

Soon after the interesting speculations of the Count appeared, I began to investigate the subject; and, by a pretty long train of experiments, which I have annually taken an opportunity of detailing in my lectures, satisfied myself that he assigned to fluidity a character that does not belong to it. Though since the date of these experiments, the public has become possessed of several series, well devised, and, in my opinion, of themselves conclusive, it may yet be worth while to state the tenor and result of them, by which the value of their testimony in favour of the conducting power of liquids may be estimated.

THE experiments were of two descriptions.

THE one set, of the same nature nearly with those of Count RUMFORD, was designed to examine, Whether heat, when applied to the surface, can descend in a fluid; and the other to discover, Whether, on the mixture of different portions of fluid at different temperatures, an interchange of caloric takes place between the particles;—Water, oil and mercury, having been the subjects of the Count's experiments, were employed for the first set.

To explore the conducting power of water and oil, the apparatus which I used consisted of two vessels of tinned iron, both cylindrical, and the one somewhat larger than the other. The larger had a diameter of eleven inches, and into it were poured the subjects of the trial, to different depths on different occasions. The smaller was ten and a half inches in diameter. By three hooks it was suspended within the larger pan, in such a manner, that the bottom of it exactly reached and came in contact with the surface of the fluid. This smaller vessel became the source of the heat, by being filled with boiling hot water. The water was changed frequently, care being taken to avoid, by the use of a syphon, all agitation and disturbance.

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but extremely slowly, by reason that fluids, as Count RUMFORD taught us, are excessively bad conductors of heat, and so very slowly,

IN experiments of this nature, the difficulty is to prevent the conveyance of caloric by the sides of the vessel. I attempted, and, I think, I succeeded, in overcoming this difficulty, by encircling the larger vessel, at a height exactly corresponding with that of the surface of the fluid within, with a gutter or channel about half an inch in depth; and by causing a stream of cold water to flow constantly through a syphon into this gutter, while from the opposite side it ran off by a small spout.

THE water was several degrees colder than the subject of the experiment; and keeping cool the portion of the vessel with which it was in contact, it intercepted the heat that would otherwise have travelled by this route to the bottom.

FOR mercury I had recourse to vessels of glass.

IN all the experiments a thermometer bore testimony that the caloric descended from the surface to the bottom of the fluid, and demonstrated, at least to my conviction, that fluids can conduct heat.

THE progress of the heat, however, was very slow, and attested the important fact, for which we ought to be thankful to the Count,—That fluids are very bad conductors.

THE second set of experiments was calculated to examine, in a very different manner, the position, That all interchange and communication of heat between the particles of fluids is impossible.

WHEN a hot and a cold fluid are mixed together and well agitated, very soon an uniform temperature is produced. This equality must proceed either from a communication of heat from the warmer to the colder fluid, agreeably to the common opinion, or from a perfect intermixture of hot and cold particles, according to the notion of Count RUMFORD. To which cause it ought to be attributed, I conceived I might discover, by ascertaining whether, after such an intermixture, any separation of the hot and cold portions took place. If the equilibrium of temperature be owing to intermixture without interchange of caloric, the hotter particles, as soon as the agitation ceases, ought, by reason of their greater rarity, to accumulate, to a certain degree, at the surface, and there exhibit a temperature above the common one.

I FIRST tried water, and mixing this fluid boiling-hot, with an equal quantity nearly ice-cold, in a stoppered glass jar, I shook them well for a short time.

I THEN noticed the resulting temperature, and raising the ball of the thermometer towards the surface, I had an opportunity of observing, that it never rose  
the

slowly, that the caloric entered from the atmosphere with sufficient quickness to prevent any depression of temperature below the 39th degree.

THIS experiment, I may conclude with remarking, is very well calculated to exhibit the error of the popular opinion, that "heat has a tendency to ascend."

#### EXPERIMENT

the smallest portion of a degree above the common temperature which had been established.

I NEXT made a similar experiment with alcohol, selecting it on account of its remarkable dilatability. I took well, for half a minute, a mixture of equal parts of alcohol at temperature  $40^{\circ}$  and at temperature  $170^{\circ}$ . The resulting temperature of the mass was  $104^{\circ}$ .

Now, if this was a mixture of particles at  $40^{\circ}$  and at  $170^{\circ}$ , as the difference of specific gravity between the fluid at these temperatures is very considerable, some separation of the warmer and lighter particles from the others, ought, I conceive, to have taken place. The temperature of the top, however, never indicated the arrival of warmer particles. It never ascended above the point of equilibrium.

FROM these experiments I concluded, that the uniformity of temperature was established by an actual communication and interchange of heat between the particles.

It may not, however, be improper to state, that Count RUMFORD, with whom several years ago I had the pleasure of conversing upon this subject, alleged, that the intermixture might be so complete as to prevent any separation whatever.

If it be a property essential to fluidity, that heat cannot pass from one particle to another, the particles of different fluids ought to be equally incapable of imparting caloric mutually to each other. Unfortunately, however, for the speculation, the caloric is so communicated. Though, *à priori*, I entertained no doubts respecting the result of the experiment, I poured a quantity of olive oil which had been heated by immersion in a vessel of boiling water for half an hour, upon an equal volume of water of  $38^{\circ}$ , and agitated the mixture, by shaking for a quarter of a minute. The common temperature produced was  $78^{\circ}$ , and the heat had gone from the oil into the water; for when the fluids separated, and had arranged themselves according to their specific gravity, both of them had the same temperature of  $78^{\circ}$ .

THE experiments of the two descriptions now recorded, left on my mind little doubt that the Count had overstrained his conclusions.

EXPERIMENT IV.

I TOOK the same tall jar, and stopping the tubulature with a cork, I filled it with water of temperature  $40^{\circ}$ , and placed it in a pan. After suspending two thermometers, as in experiment first and second, I poured a mixture of ice and salt into the pan, to the depth of 4.2 inches, the air of the room being  $40^{\circ}$ , as in the last experiment.

	Bottom.	Top.	Air.
Eleven o'clock,	$40^{\circ}$	$40^{\circ}$	$40^{\circ}$
In 10 minutes,	38+	38+	
— 20 ———	38—	38—	
— 30 ———	37—	37—	
— 40 ———	36	36	
— 60 ———	35.5	35.5	
— 80 ———	35	35	
— 100 ———	34.5	35	
— 120 ———	34—	34	
— 8 hours,	34—	34	

A CRUST of ice began to form on the inside of the glass when the water in the axis of the bottom and of the top was at  $36^{\circ}$ . In the course of the experiment, it became at least an inch thick.

WE learn from this experiment, that cold applied to the lower part of a cylinder of water, nearly 18 inches long, and having the temperature of  $40^{\circ}$ , is actually as speedily perceived at the summit as in the axis of that part, on the external surface of which it immediately acts. As fluids conduct heat so very tardily, this can only arise from currents of cooled water ascending

ing from the bottom, and these cold currents cannot move upwards, were not the water of them specifically lighter than that of the incumbent warmer fluid.

THE water, therefore, which at the bottom is cooled by the contiguous frigorific mixture, must be expanded by the loss of caloric.

THIS experiment secures full force to the last, as it obviates the objection already noticed, and also precludes another. I have already stated, that it may perhaps be alleged, that the fluid at the top, in experiment third, though cooled to  $32^{\circ}$  did not descend, because below  $40^{\circ}$  the contraction is so trifling, that it does not occasion a difference of specific gravity sufficiently great to cause the particles to descend, when opposed by the inertia and tenacity of the fluid through which they have to fall; or it may be conceived, that the descent is so tardy, that time is given to the ambient air or subjacent fluid to furnish heat enough to raise the temperature of the descending stream, and by that arrest it in its downward course.

BUT from the particulars above recorded, it is manifest, that the change of density between the temperature of  $32^{\circ}$  and  $40^{\circ}$  is quite sufficient to put into motion the particles, and to enable them to overcome the obstacle arising from inertia and tenacity, and to withstand the arresting effects of atmospheric heat.

THOUGH these experiments, and some others of a similar nature, carried conviction to my mind, and perfectly satisfied me respecting the reality of the anomaly of water, I determined to vary somewhat the mode of making the experiment, so as to obtain still more striking results.

FOR the fifth experiment, I used an apparatus which consisted of a still taller jar. It was 21 inches high, and 4 in diameter. I adjusted at the middle of its height a perforated basin of tinned iron, 2 inches in depth, and 10 in diameter. As this basin embraced



embraced the middle of the jar, I could, by filling it with hot water, or a frigorific mixture, apply heat or cold to the middle portion of the fluid in the jar, and thence, by the thermometer, learn what course the heated or cooled fluid should take.

# EXPERIMENT V.

I FILLED the jar with water at  $32^{\circ}$ . I placed it upon several folds of thick carpet, previously cooled to the same degree. The air of the room going from  $33^{\circ}$  to  $35^{\circ}$ , I introduced two thermometers, as in experiments first and second. I then poured water of temperature  $68^{\circ}$  into the basin, and by means of a spout arising from the side of it, and a syphon connected with a reservoir of water at the temperature now mentioned, I renewed the contents of the basin frequently, but without causing any agitation.

	Bottom.	Top.	Air.
At commencement,	$32^{\circ}$	$32^{\circ}$	$33-35$
In 10 minutes,	35	32	
— 15 —	36	32	
— 20 —	36+	32	
— 25 —	37	33	
— 30 —	38	33	** From this time I charged the basin with water of temperature $88^{\circ}$ , and renewed it frequently.
— 38 —	38+	33	
— 45 —	39	33	
— 50 —	39+	44	
— 55 —	39+	45	
— 60 —	39+	48	

NOTHING can be more decisive with regard to the question in dispute, than the particulars of this experiment. Heat is applied to the middle of a column of ice-cold water. The heated portion

tion has an equal share of the column of cold fluid above it and beneath it. There is nothing to determine its course in one direction or another, excepting its actual change of density.

THE thermometer evinces that the warm current sets downwards, and carries the increased temperature to the bottom. There, this instrument indicates the successive rise of several degrees, before the surface indicates the smallest acquisition of heat.

THE inference is plain, that the cold water is contracted by the heat.

THE change of the effect of heat is equally well illustrated by this experiment.

No sooner did the inferior portion attain the temperature of  $39^{\circ}$ , than the heated fluid altered its course, and, by ascending, carried the increase of temperature very rapidly to the surface, so that it soon surpassed the bottom, and continued to rise, while the other remained stationary.

#### EXPERIMENT VI.

I FILLED the jar used in the last experiment with water of temperature  $39\frac{1}{2}^{\circ}$ , the air and the support being at  $39^{\circ}$ . Disposing the thermometers in the usual manner, I introduced a mixture of snow and salt into the basin.

	Bottom.	Top.	Air.
At commencement,	39.5	39.5	$39^{\circ}$
In 10 minutes,	39+	38+	
— 25 ———	39+	36.5	*** At this time ice began to be formed on the side of the vessel.
— 35 ———	39	36—	
— 55 ———	39	35	
— an hour and 10 min.	39—	34+	
— ——— 35 ———	39—	34—	
— 2 hours,	39—	33+	

THIS

THIS experiment speaks in as decided language as the preceding. It shows that when a portion, in the middle of a column of water at temperature  $39.5$  is cooled, the colder fluid rises, and does not descend through the warmer mass, and presents the unequivocal demonstration, that water of temperature  $39\frac{1}{2}^{\circ}$  is actually expanded by losing heat.

THE different experiments which I have in detail recorded, agree perfectly with each other in the evidence they give relative to the subject of inquiry. The general import of them is, that water which is ice-cold, or a few degrees warmer, when heated, becomes specifically heavier,—that water of  $40^{\circ}$  when heated becomes specifically lighter,—that water above  $40^{\circ}$ , by the loss of heat, or by cold, is rendered specifically heavier; and that water below  $40^{\circ}$  is, by the same cause, rendered specifically lighter.

SUCH being the general import, the conclusion is irresistible, that heat, in low temperatures, causes water to contract, and at superior temperatures to expand. The opinion, therefore, is founded in truth, that water possesses a peculiarity of constitution in relation to the effects of caloric, and that it is, within a short range of temperature, an exception to the general law of “expansion by heat.”

So far as I can judge from these experiments, I am disposed to believe that the point at which the change in the constitution of this fluid in relation to heat takes place, lies between the  $39\frac{1}{2}^{\circ}$  and the 40th degree.

I AM not at present aware of any objection to the method I have followed in establishing this singular anomaly, and in removing any doubts which may have arisen from the unavoidable influence which the instrument must have in the mode of conducting the investigation that had previously been adopted.

THE plan of operation above described, however, only ascertains the fact; it gives no data for ascertaining the amount of the anomalous effect of heat.

I HAVE already stated, that M. DE LUC alleged, that from the temperature of  $41^{\circ}$ , the expansion occasioned by cold was very nearly equal to that produced by the same number of degrees of heat; and consequently that water possesses the same density at any given number of degrees of temperature above and below  $41^{\circ}$ . The first experiments of Mr DALTON appeared to confirm this opinion, and to enlarge the range to which it applied, by extending it to temperatures as far below  $32^{\circ}$ , as water allows itself to be cooled before it begins to freeze. From one circumstance that constantly occurred, I am inclined to think, that the amount of the dilatation by cold is inferior to that caused by heat.

DURING the heating or cooling of water below  $40^{\circ}$ , the difference of temperature between the top and bottom of the fluid was less than what occurred during the cooling or heating of the fluid through the same number of degrees above it; and I conceive that, when other circumstances, but particularly the rate of the change, are alike, the difference of temperature between the upper and lower parts of the fluid, as it depends upon, may prove a measure of, the difference of density.

ALCOHOL, when heated or cooled, presents, by reason of its greater expansibility, a greater difference of temperature in these situations than water; and upon the same principle I infer, that water from  $40^{\circ}$  is more expanded by an equal number of degrees of elevation than of depression.

As the concurrence of the testimony of the experiments above related with the general opinion, will probably remove every doubt respecting the matter of fact, it remains a very difficult problem for those who are fond of philosophical investigation, to explain how heat shall occasion in the same fluid, without producing any alteration of mechanical form or chemical condition, at one time contraction and at another expansion, and to reconcile the contractive effect to the conceived notions of the mechanism of the operations of this energetic agent.

WHEN

WHEN heat causes expansion, it is imagined to act by inducing a repulsion among the particles of bodies, which, opposing and overpowering the cohesive attraction, causes the particles to recede.

IN what manner, then, the addition of heat can occasion, or allow, the particles of water to approach each other, and how the subtraction of it can make them retire to a greater distance, I confess I can in no measure comprehend.

AN explanation, abundantly plausible at first view, very readily suggests itself to every one who is aware of the great and forcible expansion which happens to this fluid at the moment of its congelation. It is stated by Sir CHARLES BLAGDEN, in the paper already quoted:

THE remarkable dilatation which water experiences at the instant of being converted into ice, is very generally ascribed, and I presume very properly, to a new arrangement which the particles assume, determined probably by their polarity; by which one side of the particle A is attractive of one side of B, while it is repulsive of another.

Now, if this polarity operates with so much energy as to impart almost irresistible expansive force at temperature  $32^{\circ}$ , it is reasonable to suppose that it may begin to exert its influence, though in a far inferior degree, at temperatures somewhat more elevated. The expansion, therefore, that takes place, during the fall of temperature from  $40^{\circ}$ , may be imputed to the particles beginning or affecting to assume that new arrangement which their polarity assigns them, in which arrangement these particles occupy more space than before.

AGAIN, when heat causes water of  $32^{\circ}$  to contract, upon the same principle, it may be conceived to operate, by counteracting the small portion of the disposition to polarity that survives the liquefaction.

I AM afraid that we cannot rest satisfied with this explanation. We must not be deceived by the plausibility of it.

THE state of perfect fluidity depends upon the circumstance, that the particles of any body admit of ready motion upon each other, and that the change of relative situation meets with little or no sensible resistance.

WATER certainly possesses fluidity in a great degree, and its particles must of course encounter but little resistance, as they glide the one upon the other. But if these particles shall begin to exert any degree of polarity, by which certain faces become more disposed to attach to each other than certain others, this tendency would necessarily oppose that indifference with regard to position, which is essential to fluidity, and of course must impair the fluidity, and induce some degree of tenacity or viscosity.

To appearance, however, water at  $32^{\circ}$  has its fluidity as perfect as at temperatures considerably elevated. Unwilling to trust to appearance, where experiment might decide, I have attempted in various ways to ascertain whether the water suffers any sensible diminution in this respect while it is expanded by cold. The following method I deem the most correct.

FOR the purpose, I employed a gravimeter, the one contrived by Mr NICHOLSON for discovering the weight and specific gravity of solids.

THIS is a convenient instrument, but, unfortunately, it is by no means so ticklish as a balance. Duly loaded, so as to be equiponderant with the water in which it is plunged, Mr NICHOLSON says, it is sensible to the 20th part of a grain. The one I have, though its stem be slender, is scarcely sensible to less than two or three twentieths of a grain.

THE want of sensibility in the gravimeter arises, in a great measure, though not entirely, from a certain degree of tenacity subsisting among the particles of the fluid; and any thing that tends

tends to increase this tenacity, must, in the same proportion, augment this want of sensibility.

To ascertain whether any sensible change in the tenacity or fluidity accompanies the expansion of water by cold, which the theory requires, I examined the mobility of the instrument when immersed in water at different temperatures. I first plunged it into this fluid, heated to between  $60^{\circ}$  and  $70^{\circ}$ . Under due loading, which sunk it to the mark on the stem, it was not sensible to a weight less than two or three twentieths of a grain.

I THEN tried it in ice-cold water, and found that its sensibility was in no perceptible degree impaired. The coldness of the water, it must be remembered, causes some degree of contraction of the gravimeter. This contraction cannot fail to render the instrument in some small measure more sensible, and, so far as it goes, to counteract the sluggishness produced by any increased tenacity in the fluid.

BUT as the body of the instrument is made of glass, the amount of the contraction must be very small, and the change of sensibility arising from it so very trifling, as certainly by no means to obscure such an effect as an increase of tenacity would occasion. I therefore with some confidence conclude, that the fluidity of the water is not sensibly diminished, and consequently that the polarity has not begun to exert any sensible influence; it can scarcely, therefore, be accounted the cause of the dilatation.

*END OF PART SECOND.*

...the body of the instrument is made of glass, the  
...of the connection must be very small, and the change  
...of sensibility arising from it is very trifling, so that by no  
...to obscure such an effect as an increase of tenacity would  
...I therefore, with some considerations, conclude, that the  
...of the glass is not sensibly diminished, and consequently  
...the polarity has not begun to exist any longer, and that  
it can barely, therefore, be accounted the cause of the discon-  
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# H I S T O R Y

OF

## THE SOCIETY.

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**I**T has been thought more consistent with the form which the Transactions of the Society have assumed in this volume, to arrange the abstracts and accounts of papers in the order of the subjects, than in the order of time. Under each head, the papers are placed according to the dates of their communication.

### METEOROLOGY.

A COMPARISON of some Observations on the Diurnal Variations of the Barometer, made in PEYROUSE's Voyage round the world, with those made at Calcutta by Dr BALFOUR, was read by Professor PLAYFAIR.

1799.  
Jan. 7.  
Diurnal variations of the barometer.

THE first of the observations here referred to were made by M. LAMANON, an ingenious naturalist who accompanied PEY-ROUSE, and who has given an account of them, (see 4th volume of the *Voyage*, 8vo edit.), in a letter to M. DE CONDORCET, dated, St Catherine, 5th November 1785. Dr BALFOUR's Observations are in the *Asiatic Researches* for 1744, and a short account of them is also inserted in the 4th volume of the *Transactions*, R. S. Edin. Hist. p. 23.

M. LAMANON's observations were made in consequence of instructions from the Academy of Sciences, directing him to keep an exact account of the heights of the barometer in the vicinity of the equator at different hours of the day, with a view to discover, if possible, the quantity of the variation of that instrument, due to the action of the sun and moon, that quantity being there probably at its *maximum*, while the variations arising from other causes are at their *minimum*.

M. LAMANON was provided with one of NAIRNE's marine barometers, which, he says, was so little affected by the motion of the ship, that it might be depended on to the  $\frac{1}{100}$  of an inch. In this barometer, he tells us, that from about the 11th degree of north latitude, he began to perceive a certain regular motion, so that the mercury stood highest about the middle of the day, from which time it descended till the evening, and rose again during the night. As they approached the equator, this became more distinctly perceptible; and on the 28th of September, the ship being then in  $1^{\circ} 17'$  north latitude, a series of observations was begun, and continued for every hour till the 1st of October, at 6 A. M. The following abstract shews the result of the observations on the 28th and 29th.

28th Sept.	{	From 4 to 10 A. M.	Barometer rose $1 \frac{2}{10}$
		From 10 A. M. to 4 P. M.	fell $1 \frac{2}{10}$
		From 4 to 10 P. M.	rose $0 \frac{2}{10}$
29th			

29th Sept.	{	From 10 (28th) to 4 A. M.	fell 1 $\frac{3}{10}$
		From 4 to 10 A. M.	rose 1 $\frac{5}{10}$
		From 10 A. M. to 4 P. M.	fell 1 $\frac{1}{10}$
		From 4 to 10 P. M.	rose 1

THE observations on the 30th were to the same effect; and hence it is concluded that at the equator the flux and reflux of the atmosphere produces in the barometer a variation of about 1 line  $\frac{2}{10}$  English, corresponding, as M. LAMANON remarks, to a height in the atmosphere of nearly 100 feet. According to BERNOUILLI, the action of the sun and moon should produce a tide of about 7 feet, and according to Mr DE LA PLACE, a tide not nearly so great.

IT should be observed, that when these observations were made, the moon was in her last quarter, and the sun a few degrees to the south of the equator. The latitude on the 28th was 50' north, and 11' north on the 29th; in the night between that and the 30th, the ship crossed the line; and on the 30th at noon, the latitude was 42' south: the longitude all this while between 17° 31' and 18° 33' west of Paris, by the time-keeper; so that the coast of Africa, which was the nearest land, was distant about 8° of a great circle, and the American continent about 19°.

THE agreement between these, and Dr BALFOUR's observations at Calcutta is very remarkable. Dr BALFOUR found that during the whole lunation, in which he observed the barometer from half-hour to half-hour, the mercury constantly fell from 10 at night to 6 in the morning; from 6 to 10 in the morning it rose; from 10 in the morning to 6 at night it fell again; and lastly, rose from 6 to 10 at night. The *maximum* height is therefore at 10 at night and 10 in the morning, and the *minimum* at 6 at night and 6 in the morning. The only difference is, that in Mr LAMANON's observations, the *minimum* is stated to have happened about 4, instead of 6. This, however, will not

not seem a very material difference, when it is remembered, that the instant when any quantity attains either its greatest or its least state is not easily ascertained with precision. From the observations as detailed by M. LAMANON, the time of the *minimum* seems to answer fully as well to 5 as to 4; so that the difference of the results is in every view inconsiderable, and their coincidence, on the whole, not a little singular. The variations in Dr BALFOUR's barometer between the nearest *maximum* and *minimum*, is sometimes about  $\frac{1}{10}$ th of an inch, though in general considerably less.

IN the abstract of Dr BALFOUR's observations referred to above, it is remarked, that it seems not improbable that these variations of the barometer are connected with the reciprocations of the sea and land winds during the day and night. But whatever may have been formerly the probability of this supposition, it is entirely destroyed by the observations of the French navigators. These observations were made too far out at sea to leave room for supposing that the land winds had any influence on the phenomena to which they refer. It is at the same time doubtful, whether those phenomena can be ascribed to the atmospheric tides produced by the sun and moon, as the *ebbing* and *flowing* of the mercury in the barometer appears to have no dependence on the position of those luminaries relatively to one another, but happens, it would seem, constantly at the same hour, in all aspects of the moon and all seasons of the year. The subject is well deserving of a fuller investigation. We should probably before now have had farther information respecting it, if happily the able navigator above named, and his brave associates, had been destined to revisit their native shores. The cruel fate of an expedition so well planned, and so well appointed for the purposes of science, will never cease to be matter of sincere regret.

AN Account of an Aurora Borealis, observed in day-light at Aberfoyle in Perthshire, on the 10th February 1799, by PATRICK GRAHAM, D. D. minister of Aberfoyle, was communicated by the Reverend Dr FINLAYSON.

1799.  
Nov. 4.  
An aurora borealis observed by day-light.

“ON the 10th of February 1799, about half-an-hour past 3 o'clock P. M., the sun being then a full hour above the horizon, and shining with an obscure lustre through a leaden-coloured atmosphere, I observed,” says Dr GRAHAM, “the rare phenomenon of an aurora borealis by day-light. The weather, for several days before, had been intensely cold; and during the two preceding days, much snow had fallen. On this day a thaw had come on, and the temperature of the air was mild. The general aspect of the sky was serene. Some dark clouds hung on the horizon between S. W. and W. I was intensely observing a large halo about the sun, of about 20 degrees in semi-diameter: It exhibited the prismatic colours, though obscurely, except in one quarter, where it coincided with the skirt of a dark cloud on the horizon, almost directly west. In that portion of the halo, the colours of the iris were very distinctly exhibited.

WHILST I was attending to this appearance, the whole visible hemisphere of the heavens became covered with a light palish vapour, as I at first imagined it to be. It was disposed in longitudinal streaks, extending from the west, by the zenith, and all along the sky towards the east. On examining this appearance more narrowly, I found it to be a true aurora borealis, with all the characters which distinguish that meteor when seen by night, excepting that it was now entirely pale and colourless. The stream of electric matter issued very perceptibly from the cloud in the west, on the skirts of which the halo exhibited the prismatic colours; thence diffusing themselves, the rays converged towards the zenith, and diverged again towards every quarter of the horizon; and the corruscations were equally instantaneous, and as distinctly perceptible as they are by night.

THIS

THIS appearance continued for more than 20 minutes, when it gradually vanished, giving place to thin scattered vapours, which, towards sun-set, began to overspread the sky. Through the ensuing night, I could not discern the smallest trace of these meteors in the sky.

THIS appearance, I find, is not altogether new. In the *Annual Register* for 1789, there is an account, extracted from the *Philosophical Transactions*, of an aurora borealis seen by day-light in Ireland, by Dr HENRY USHER, who, considering himself as the first observer of this phenomenon, requests that any person to whom a similar appearance may occur would communicate his observations. There is indeed reason to believe, that the phenomenon occurs not unfrequently; but unless it is attended to very accurately, it will in general escape observation; nor should I have at this time remarked it, had I not been engaged in observing the solar halo. Whenever the sky, being for the most part cloudless, is suffused with thin pale vapours, especially if disposed in longitudinal streaks, observers should look out for this phenomenon."

1800.  
Jan. 6.  
Phenomenon of  
two intersect-  
ing rainbows.

AN account of two intersecting rainbows, seen at Dungalass in East Lothian in July last, was communicated by Professor PLAYFAIR.

"AT Dungalass, where I happened to be in the beginning of July last, our attention was called one evening, a little before sun-set, to a very large and beautiful rainbow, formed on a cloud which hung over the sea, and from which a shower was falling at a considerable distance to the S. E. The sun was about  $2^{\circ}$  high, so that the arch was not much less than a semicircle, with its highest point elevated about  $40^{\circ}$ . At the point where the northern extremity of this arch touched the horizon, another arch seemed also to spring from the sea, diverging from the former at an angle of  $3^{\circ}$  or  $4^{\circ}$ , on the side toward the sun.

THIS

THIS arch did not exceed  $7^{\circ}$  or  $8^{\circ}$  in length; it was of the same breadth with the principal bow; it had the colours in the same order, and nearly of the same brightness; or, if any difference was discernible, it was, that the transition from one colour to another was not made with so much delicacy in the last-mentioned rainbow as in the former.

WE recollected that a phenomenon similar to this is described in the *Philosophical Transactions*, as having been seen at Spithead, and that it is ascribed by the gentleman who observed it to the reflection of the sun's rays from the surface of the sea, so as to fall on the cloud where the rainbow was formed. This hypothesis seemed to agree exactly with the phenomenon now before us.

THE accidental rainbow, for so it may be called, was seen only at the extremity where the principal arch rose from the sea, and where, of consequence, the sun's rays, reflected from the surface of the water, at that moment very smooth, might fall on the drops of rain. The other parts of the cloud could not receive rays so reflected, as the land intervened, and there, accordingly, no vestige of the accidental rainbow was observed.

THE accidental rainbow lay, as was already said, on the side toward the sun, and this is agreeable to the hypothesis; for the rays that after reflection from the surface of the water fell on the drops of rain, must have come as from a point as much depressed below the horizon, as the sun was at that instant elevated above it. The axis of the accidental rainbow must therefore have made with the axis of the principal, an angle equal to twice the sun's elevation, and its centre must have been elevated by that same quantity above the centre of the other, so that if it had been complete, it would have been wholly between the principal rainbow and the sun.

THE only circumstance in which the appearances did not perfectly correspond with this hypothesis, was, that the two rainbows did not intersect one another in the horizon, but rather a little above it. This, however, ought to have no great weight, as the reflected image of the sun cannot have presented to the cloud a disk so regular and well defined as the sun itself and the accidental rainbow must have somewhat participated of this indistinctness.

WHEN phenomena of this kind occur, it would afford a sure means of trying the justness of the explanation, if the inclination of the two bows were observed, and also the sun's altitude at the same time. These two things are necessarily connected; for if we call  $I$  the angle of their intersection,  $E$  the elevation of the sun, and  $S$  the angle subtended at the eye by the semidiameter of the rainbow, if complete, an angle which is constantly the same, and nearly equal to  $42^\circ$ , it is easy to infer from spherical trigonometry, that  $\sin \frac{1}{2} I = \frac{\sin E}{\sin S}$ .

COMPUTING from this formula, the inclination of the two bows in the present instance comes out nearly  $5^\circ$ ; somewhat greater than I was inclined to estimate it by the eye.

PHENOMENA of this kind can but rarely occur, as the necessary conditions will not often come together. The principal rainbow must be over the sea; the sea itself must extend somewhat on the side toward the sun; it must be smooth and tranquil, and the sun so low that the light reflected from the water may be considerable. Were it ever to happen that the accidental bow was completely formed, the effect could not fail to be very striking.

CHEMISTRY.



## CHEMISTRY.

SIR GEORGE MACKENZIE, Bart. read a paper containing an account of experiments which he had made on the Combustion of the Diamond.

1800.  
Feb. 3.  
On the combustion of the diamond.

WHAT follows is a short abstract of this paper; the paper itself having been published in *Nicholson's Journal* for July 1800.

THE first object of these experiments was to ascertain the temperature at which the combustion of the diamond takes place. For this purpose a diamond, together with a pyrometer of WEDGEWOOD, was placed on a piece of baked clay, and pushed gradually into the muffle: when both were perfectly red throughout, the pyrometer was withdrawn, and indicated  $13^{\circ}$ . The diamond had acquired the dim milky appearance which is known to indicate an incipient combustion. It was then replaced in the muffle, together with the pyrometer; and the heat, being slowly increased till a glow, indicating that the diamond was completely on fire, appeared, was continued, as equal as possible, till the diamond was totally consumed. The pyrometer, when measured, indicated  $14^{\circ}$ . In another experiment, the heat required to produce the glow was  $15^{\circ}$ , and at this temperature the diamond was wholly consumed.

SIMILAR experiments were repeated, with different diamonds, and with nearly the same result, which shews that the heat required for their combustion is much less than it has hitherto been supposed.

GUYTON's experiment of converting iron into steel by means of the diamond, was repeated in the following manner: Into a hollow cylinder of soft iron, closed at one end, some small diamonds were put, and a stopper of the same iron being afterwards applied, the two pieces were rivetted together at the top.

The cylinder being then placed in a Hessian crucible, and surrounded with a mixture of dry sand and clay, was subjected to the fire of a smith's forge for an hour. When taken out, the pyrometer indicated  $151^{\circ}$ ; the upper part of the iron appeared to have been melted, and several bright metallic globules were seen adhering to the compacted mass of sand and clay next the iron. The lower part of the cylinder retained its shape, but was blistered on the surface, except at the bottom, which remained smooth. When the ends were polished and touched with diluted nitric acid, they exhibited the spot characteristic of steel, that on the end which had been fused being considerably darker than the other. The part which had remained smooth after being cut off, was heated red hot, and plunged into cold water, when it became so hard, that it received no impression from a file. Several cavities were found within the cylinder, but the diamonds had totally disappeared.

THE whole cylinder of soft iron was thus converted into steel; one end of it having been melted, was in the state of cast steel; the other end, having remained solid, was of the kind of steel produced by cementation.

A GENTLEMAN of the Clyde Ironworks having about that time been led, by some experiments, to doubt of the conclusiveness of those made by GUYTON, and to suspect that the diamond had contributed nothing to the conversion of the iron into steel, Sir GEORGE MACKENZIE thought it necessary to direct some experiments to the solution of this question.

IN one of these pieces of soft iron, in all respects similar to that used in the last experiment, was exposed to the same degree of heat as in that experiment, and with all the circumstances alike, except that the diamonds were wanting. The result was, that the iron was not altered in its shape or qualities. It had been exposed to the heat an hour, and the pyrometer marked  $152^{\circ}$ .

152°. Hence it was natural to conclude, that the conversion into steel in the former experiment, is solely to be attributed to the action of the diamond. It had been suggested by the gentleman above alluded to, that when soft iron is exposed to a high temperature, carbon dissolved in caloric penetrates the crucibles, and converts the iron into steel. To bring this hypothesis to the test of experience, it was thought proper to expose the soft iron to a strong heat in an apparatus which must exclude all carbonaceous matter, except such as was so dissolved. The substance which, by being interposed between the iron and the crucibles, seemed to be most likely to answer this purpose, was the white felspar or adularia; Dr KENNEDY, in an analysis of this substance, having found that it began to be vitrified in a heat of 90°, so that there was reason to think, that before the heat was intense enough to melt the iron, the iron would be surrounded by a mass free from carbonaceous matter, and impervious also to carbon coming from without.

INTO a small crucible, made of the porcelain clay of Cornwall, was put some felspar in fine powder, and upon this a small cylinder of soft iron, after which the crucible was filled with the felspar, which therefore surrounded the iron on all sides. The crucible was then placed in an air furnace, in which the heat was gradually raised for an hour, and continued for another hour at its highest pitch, after which the crucible was withdrawn. The heat marked by the pyrometer was 152°. The iron retained its shape and all its properties unchanged. The felspar was reduced to a glass, transparent and colourless, except that it had received a greenish tinge where it was in contact with the iron.

SEVERAL other experiments were made with the same view as this last, but with the circumstances varied as much as possible, and in all the result was the same; that is, the iron, without the presence of the diamond, was never converted into steel; so that

that there appears no reason to suppose that soft iron can be converted into steel by carbon penetrating from the fuel through the crucibles.

SIR GEORGE MACKENZIE concludes his account of these very interesting experiments with the description of one which appears to be entirely new, and tends still farther to prove the identity of carbon and diamond.

HAVING prepared some pure oxide of iron from a solution of the sulphate, by precipitation with caustic ammonia, he mixed a small quantity of it with one-fourth of its weight of diamond powder, prepared in the following manner.

THE diamond, being reduced to powder in a steel mortar, was boiled in muriatic acid, to dissolve the iron which might have been abraded from it. After properedulcoration, it was heated in a muffle, to burn off the carbon of the steel, which remained after treatment with the acid, and which rendered the powder of a grey colour. He observed the coaly matter take fire at the edge of the heap of powder next the strongest heat, and gradually spread itself, till at last the whole appeared as if burning. The glow through the powder ceased soon after, and on removing it, he found it perfectly clean and white. From the diminution of the original weight of the diamond, he found that a part of it had also been consumed.

THE mixture of oxide and diamond powder thus prepared was put into a Cornish-clay crucible, and exposed to a pretty strong heat for half an hour, after which the oxide was found to be reduced into a metallic button of cast-iron \*.

ANOTHER portion of the oxide of iron, used in this experiment, was not reduced, when placed in the same circumstances, without the diamond.

#### MINERALOGY.

\* IN the course of this experiment it was ascertained, that the fusing point of iron is between  $153^{\circ}$  and  $158^{\circ}$  of WEDGWOOD'S pyrometer.

## MINERALOGY.

THE Reverend Dr WILLIAM RICHARDSON, late F. T. C. D., having sent to Dr HOPE a collection of specimens from the northern coast of Antrim, with a catalogue, and observations, the specimens were exhibited, and the observations were read in the Royal Society, March 1803.

1803.  
March 7.  
Remarks on the  
basaltes of the  
Coast of An-  
trim.

## SILICEOUS BASALT.

DR RICHARDSON discovered the fossil to which he gives this name in the peninsula of Portrush, four or five years ago. It abounds also in the Skerry islands, a reef of rocky islets extending from the northern point of Portrush-head for about a mile eastward. A small part of every one of those islets is formed of this stone, while the remainder consists of coarse basalt, similar in all respects to that on the east side of the above-mentioned peninsula. It is met with in one or two other places.

THIS stone is arranged in strata, from ten to twenty inches thick, all steadily parallel to one another, and every stratum, as far as can be observed, preserving an uniform thickness through its whole extent. When these strata are quarried into, they appear to be constructed of large prisms, generally pentagonal, which when broken divide into smaller prisms. This internal prismatic construction frequently gives an irregular or shivery appearance to the fracture, which however is often conchoidal, and the grain as uniform as in the Giant's Causeway basaltes.

THE beds of this fossil are remarkable for containing marine exuviae in great abundance, particularly impressions of *cornua ammonis*. The flat shells and impressions contained in these stones are steadily parallel to each other, and perpendicular to the axis of the prisms. It must be observed, that the prismatic construction is never interrupted by the shells dispersed through

it; the planes which separate the prisms passing equally through the shells and the stone itself.

THE grain of this stone passes by insensible shades from a high degree of fineness, until it become undistinguishable from that of the common columnar basaltes.

THE name of Siliceous Basalt, which Dr RICHARDSON employs, was first given to this fossil by Mr PICTET of Geneva, when he visited Portrush, in a tour through Ireland two years ago. He considered it as a variety of basalt, containing a greater proportion of *silica* than usual.

THE strata of siliceous basalt, both at Portrush and the Skerry islands, generally alternate with strata of equal thickness of a coarse-grained basalt of a grey colour. The materials of the strata grow into each other, so as to form one solid mass, from which it is easy to quarry pieces in the confine of the two strata, with a part of each adhering; but the coarse basalt, as it approaches very near to the fine, always abates somewhat of its coarseness; yet the line of demarcation is left completely distinct.

THE peninsula of Portrush lies about six miles to the west of the Giant's Causeway, and on its eastern surface alone presents these strata.

IN the space of about 700 yards, it exhibits in miniature those changes and interruptions of the strata, which occur on the large scale along the northern basaltic coast of Ireland. At the place where it emerges from the strand, there first occurs a mass composed of strata of the coarse and siliceous basalt, placed over each other alternately; this is succeeded by an accumulation of regular strata of the coarse basalt alone. A second alternation, and a second accumulation of the coarse-grained strata, come in order, and extend to the well called Tubber Wherry. Here commences an accumulation of many strata of the siliceous basalt alone, which stretches along the shore for about 100 yards, and then changes into a third alternation, which continues

continues

neus to the little boat-harbour, called *Port-in-too*, near which the filiceous basalt disappears. Over this stretch, notwithstanding the frequent change in the arrangement of the strata, the thickness of each stratum, of both species, remains pretty nearly the same, and the position of them all steadily so, viz. with a considerable dip to E. N. E.

THE west side of the peninsula, though only about 400 yards distant, consists entirely of coarse basalt. It shows a bolder face, and is formed of rude massive pillars from 60 to 80 feet long.

"I AM aware," says Dr RICHARDSON, "that several mineralogists deny the shell-bearing stone to be basalt, while others contend strenuously that it is. I will not venture to decide on the question, but must remark, that I have never met with it but contiguous to basalt, and so solidly united to this last, that the continuity of the whole mass was uninterrupted. The grain of the stone graduates, as has been already remarked, into that of the common basaltes; and the arrangement of it, and that of the basalt, with which it is so much mixed at Portrush and the Skerry islands, is exactly the same; the strata of each scarcely differing in thickness, and not at all in inclination. The strata of both kinds break into prisms, and the surfaces, where accessible, exhibit the appearance of causeways, differing only in this, that in the filiceous basalt, the pentagon is the prevalent figure, and in the coarse basalt, the quadrangle. The fusibility of both stones is also nearly the same; the shells in the filiceous basalt are calcined in the fire, and many more are then discovered which had before escaped the eye \*."

C 2

WHINSTONE

\* DR RICHARDSON observes, that some mineralogists deny that this fossil is basalt. Several of the members present when this paper was read, some of whom had examined the stone in its native place, were of that number. It was remarked, that though certain portions of the strata of this fossil bore much resemblance to some species of basalt, by far the greater part of the mass bore no resemblance whatever to any.

IT was also stated, that the substance of the coarse-grained, undisputed basalt, which lies between the strata of this stone, does not contain any vestiges of marine animals;

## WHINSTONE DIKES ON THE COAST OF ANTRIM.

DR RICHARDSON describes some particulars in the construction of the whinstone dikes on the coast of Antrim, which appear singular, and deserving of attention. These dikes, he says, are uniformly formed of large massive prisms laid horizontally, which are always divisible into smaller prisms that are likewise horizontal. To prevent confusion, he calls the first of these *component prisms*, and the second, or smaller ones into which the others break, *constituent prisms*.

THE component prisms are sometimes of enormous size, and in the same dike are nearly equal; the constituent prisms are small, (the sides about an inch long), and neatly formed.

THE dike which traverses the Giant's Causeway, differs from those on other parts of the coast, by having no component prisms. It resembles a plain wall, of which the parts shiver under the hammer into very neat constituent prisms. In the dike at Seaport the same thing is observed; the prismatic structure does not penetrate two inches from its edge; the whole interior seems an amorphous mass.

THE specimens of this latter dike, sent to Dr HOPE, exhibit its continuity with the adjacent basaltic rock which it traverses, and also the continuity of the fine basalt of its edge with the *granular* stone which composes the middle of the dike.

THE dike of *Port-coan* is a very solid mass, composed of stones apparently round, and imbedded in a basaltic paste, or indurated mortar. The round stones are formed of concentric spheres,

animals: That veins often issue from the beds of this real basalt, and pervade the supposed siliceous species; some of them connecting together the separate beds of the real basalt; others dying away in slender ramifications, as they rise through the interposed stratum. In no instance is this reversed: The veins never proceed from what is called the Siliceous Basalt. It was farther observed, that both the fracture and external surface of this stone exhibit a stratified structure, in many instances, which never happens in the true basaltes.



spheres, like the coats of an onion; they exceed a foot in diameter, and, together with the mortar by which they are united, they form a very compact and highly indurated rock.

BESIDES these large dikes, Dr RICHARDSON remarks, that veins from half an inch to an inch and a half thick, often cut the basaltic strata on that coast in all directions. The materials of these veins are never the same with the contiguous basalt, but are generally finer. At Portrush is a large vein, and near it a smaller vein, not an inch thick, which, proceeding from below, terminates in the solid rock before it reaches the surface.

#### MISCELLANEOUS OBSERVATIONS.

SOME of the specimens in Dr RICHARDSON'S catalogue are from a quarry in a mass of basalt at Ballylughan, two miles south of Portrush. This basalt contains small cavities in its interior; many of them full of fresh water, which gushes out when the stone is broken by the hammer, as if it had been in a state of compression. The stone is so hard, and flies so in pieces, that Dr RICHARDSON has not been able to collect any of the water for the purpose of analysis.

THE face of the quarry in which this variety of the basalt is found is about 15 feet high, and is cut into a stratum, the thickness of which is not yet ascertained. The rock is entirely columnar, the pillars somewhat smaller than those of the Giant's Causeway, less perfect, not articulated, sometimes bent, and variously inclined. The sides and the interior of the pillars are full of cavities. In consequence of the observations of Dr HAMILTON and Mr WHITEHURST respecting the porous texture of the air or bladder holes of the basaltes of the Causeway and its vicinity, Dr RICHARDSON has examined a great variety; but in no instance, except this of Ballylughan, has he found  
cavities,

cavities in the interior of the basaltic rocks on this coast, though they are frequent on the surface exposed to the air.

THE last variety of whinstone enumerated by Dr RICHARDSON is the Ochrous, which makes, as he says, a conspicuous figure in the stupendous precipices along the coast of Antrim. It is disposed in extensive strata of every thickness, from an inch to twenty-four feet, and varies in colour, from a bright minium to a dull ferruginous brown.

THREE remarks are made by Dr RICHARDSON, that are undoubtedly of importance, and show that this stone is merely basalt in a certain state of decomposition.

1. THE ochrous strata are extensive ; they remain always parallel to the basalt strata which they separate ; they unite to the basalt without interrupting its solidity ; the change from the one to the other is sudden, and the lines of demarkation are distinct. The ochrous stone is never found but contiguous to other basalt.

2. THE substances imbedded in the ochrous rock, and in basalts, are exactly the same ; calcareous spar, zeolite, chalcedony, &c.

3. AMONG the varieties which this rock presents, there may be found every intermediate stage between sound basalt and perfect ochre. The change is often partial, beginning with veins and slender ramifications.

## ALGEBRA.

RULE for reducing to a Continued Fraction the Square Root of any given Integer Number, not a Square. By JAMES IVORY, Esq. Communicated 10th January 1801.

1. LET  $N$  be the given integer number, and take  $n$  the root of the square next less than  $N$  ; and, for the sake of uniformity, - put  $P^{\circ} = 1$ ,  $R^{\circ} = 0$ ,  $\mu = n$ .

2. TAKE

1800.

Jan. 10.

Rule for reducing a square root to a continued fraction.

2. TAKE  $P' = N - n^2$  for a divisor, and  $2n - R^0 = 2n$  for a dividend. Let the quotient be  $\mu'$ , and remainder  $R'$ .

3. TAKE  $P'' = P' - (R^0 - R')\mu'$ , for a divisor, and  $2n - R'$  for a dividend. Let the quotient be  $\mu''$  and the remainder  $R''$ .

4. TAKE  $P''' = P' - (R' - R'')\mu''$  for a divisor, and  $2n - R''$  for a dividend. Let the quotient be  $\mu'''$ , and the remainder  $R'''$ .

5. THESE operations may be continued without end; the divisor  $P^p$  being found from the formula  $P^p = P^{p-2} - (R^{p-2} - R^{p-1}) \times \mu^{p-1}$ : the corresponding dividend being  $2n - R^{p-1}$ ; and the quotient of the division being denoted by  $\mu^p$ , and the remainder by  $R^p$ . But it will only be necessary to continue these operations till we arrive at a value  $P^p = P^0 = 1$ , which will always necessarily be the case. After this, the series of numbers,  $\mu^{p+1}, \mu^{p+2}, \mu^{p+3}$ , will necessarily be the same as the numbers  $\mu', \mu'', \mu''', \&c.$ ;  $\mu^p$  continually repeated in their order.

THE rule may be shortly expressed in algebraic language, thus:

$$\begin{array}{ll} P^0 = 1; & \mu = n \times 1 + R^0 = n; \\ P' = N - n^2; & 2n - R^0 = 2n = P' \times \mu' + R'; \\ P'' = 1 - \mu'(R^0 - R') = 1 + \mu'R'; & 2n - R' = P'' \times \mu'' + R''; \\ P''' = P' - \mu''(R' - R''); & 2n - R'' = P''' \times \mu''' + R'''; \\ P^{iv} = P'' - \mu'''(R'' - R'''); & 2n - R''' = P^{iv} \times \mu^{iv} + R^{iv}; \\ \text{and so on.} & \end{array}$$

HAVING thus found the numbers,  $\mu, \mu', \mu'', \mu^p$ , we shall have the continued fraction sought,

$$\sqrt{N} = \mu + \frac{1}{\mu'} + \frac{1}{\mu''} + \frac{1}{\mu'''} + \&c. \frac{1}{\mu^p}$$

And the fraction may be continued indefinitely, by repeating the denominators  $\mu', \mu'', \dots \mu^p$ , continually in their order.

EXAMPLE

EXAMPLE I. To reduce the square root of 13 to a continued fraction.

THE operation will be as under :

$$N = 13, n = 3, 2n = 6.$$

$$P^0 = 1, \mu = 3, R^0 = 0.$$

$$P' = 13 - 9 = 4; \quad \frac{6 - R^0}{4} = \frac{6}{4} = 1 + \frac{2}{4}; \quad P' = 4, \mu' = 1, R' = 2.$$

$$P'' = 1 + \mu'R' = 1 + 2 = 3; \quad \frac{6 - R'}{3} = \frac{4}{3} = 1 + \frac{1}{3}; \quad P'' = 3, \mu'' = 1, R'' = 1.$$

$$P''' = 4 - 1 \times (2 - 1) = 3; \quad \frac{6 - R''}{3} = \frac{5}{3} = 1 + \frac{2}{3}; \quad P''' = 3, \mu''' = 1, R''' = 2.$$

$$P^{iv} = 3 - 1 \times (1 - 2) = 4; \quad \frac{6 - R'''}{4} = \frac{4}{4} = 1 + \frac{0}{4}; \quad P^{iv} = 4, \mu^{iv} = 1, R^{iv} = 0.$$

$$P^v = 3 - 1 \times (2 - 0) = 1; \quad \frac{6 - R^{iv}}{1} = \frac{6}{1} = 6 + \frac{0}{1}; \quad P^v = 1, \mu^v = 6, R^v = 0.$$

Here I stop, because,  $P^v = P^0 = 1$ ; and I conclude that the fraction sought is formed by the numbers,  $\mu', \mu'', \mu''', \mu^{iv}, \mu^v$ ; that is, by the numbers 1, 1, 1, 1, 6, continually repeated in their order. Thus,

$$\sqrt{13} = 3 + \frac{1}{1 + \frac{1}{1 + \frac{1}{1 + \frac{1}{1 + \frac{1}{6 + \frac{1}{1} \&c.}}}}}$$

EXAMPLE II. To reduce  $\sqrt{61}$  to a continued fraction :

$$N = 61, n = 7, 2n = 14.$$

$$P^0 = 1, \mu = 7, R^0 = 0.$$

$$P' = 61 - 49 = 12; \quad \frac{14 - R^0}{12} = \frac{14}{12} = 1 + \frac{2}{12}; \quad P' = 12, \mu' = 1, R' = 2.$$

$$P'' = 1 + 1 \times 2 = 3; \quad \frac{14 - 2}{3} = \frac{12}{3} = 4 + \frac{0}{3}; \quad P'' = 3, \mu'' = 4, R'' = 0.$$

$$P''' = 12 - 4 \times (2 - 0) = 4; \quad \frac{14 - 0}{4} = \frac{14}{4} = 3 + \frac{2}{4}; \quad P''' = 4, \mu''' = 3, R''' = 2.$$

$$P^{iv} = 3 - 3 \times (0 - 2) = 9; \quad \frac{14 - 2}{9} = \frac{12}{9} = 1 + \frac{3}{9}; \quad P^{iv} = 9, \mu^{iv} = 1, R^{iv} = 3.$$

$P^v$

$$P^v = 4 - 1 \times (2 - 3) = 5; \frac{14-3}{5} = \frac{11}{5} = 2 + \frac{1}{5}; P^v = 5, \mu^v = 2, R^v = 1.$$

$$P^{vi} = 9 - 2 \times (3 - 1) = 5; \frac{14-1}{5} = \frac{13}{5} = 2 + \frac{3}{5}; P^{vi} = 5, \mu^{vi} = 2, R^{vi} = 3.$$

$$P^{vii} = 5 - 2 \times (1 - 3) = 9; \frac{14-3}{9} = \frac{11}{9} = 1 + \frac{2}{9}; P^{vii} = 9, \mu^{vii} = 1, R^{vii} = 2.$$

$$P^{viii} = 5 - 1 \times (3 - 2) = 4; \frac{14-2}{4} = \frac{12}{4} = 3 + \frac{0}{4}; P^{viii} = 4, \mu^{viii} = 3, R^{viii} = 0.$$

$$P^{ix} = 9 - 3 \times (2 - 0) = 3; \frac{14-0}{3} = \frac{14}{3} = 4 + \frac{2}{3}; P^{ix} = 3, \mu^{ix} = 4, R^{ix} = 2.$$

$$P^x = 4 - 4 \times (0 - 2) = 12; \frac{14-2}{12} = \frac{12}{12} = 1 + \frac{0}{12}; P^x = 12, \mu^x = 1, R^x = 0.$$

$$P^{xi} = 3 - 1 \times (2 - 0) = 1; \frac{14-0}{1} = \frac{14}{1} = 14 + \frac{0}{1}; P^{xi} = 1, \mu^{xi} = 14, R^{xi} = 0.$$

HERE I stop, because  $P^{xi} = P^0 = 1$ . And the series of numbers sought is, 1, 4, 3, 1, 2, 4, 1, 3, 4, 1, 14.

ON turning to page 378 of the English edition of EULER'S *Algebra*, it will be found that the table there given consists of the two series of numbers,  $P^0, P', P'', \&c.$  and  $\mu, \mu', \mu'', \mu''', \&c.$

THIS rule is the more worthy of notice, that it proceeds by certain definite arithmetical operations: whereas the method of M. DE LA GRANGE determines the numbers,  $\mu, \mu', \mu'', \&c.$  by appreciating the value of certain expressions to the nearest unit, or by a process that is in some measure tentative, and therefore not strictly analytical.

## SURGERY.

MR RUSSEL read an account of a singular variety of Hernia which occurred to him while he was delivering clinical lectures in conjunction with Dr BROWN and Mr THOMSON. Mr THOMSON dissected the parts with great care and accuracy, and discovered certain peculiarities, which makes the knowledge of this

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D

variety

1803.  
Mar. 7.  
A singular  
variety of her-  
nia.

variety a real addition to the pathology of the disease. It is a modification of Inguinal Hernia. But as the circumstances of difference are constant and essential, it may fairly be regarded as a distinct species. In the common cases of inguinal hernia, the viscera which are to form the protrusion enter the upper and internal orifice of the abdominal ring, along with the spermatic cord, accompany the cord through the whole length of the passage, and come out along with it at the inferior and external orifice of the ring. Thus the hernia is formed by the dilatation of a natural passage. But in this new and hitherto undescribed variety, the viscera burst through the common parietes of the abdomen, exactly opposite to the lower and external orifice of the ring, where they come into contact with the spermatic cord, and descend along with it directly into the scrotum. This hernia, therefore, resembles a ventral hernia in its commencement, by beginning to protrude where there is no natural opening; and it resembles an inguinal hernia, by passing through the lower and external orifice of the abdominal ring, where the protrusion of the common inguinal hernia is completed. Thus it is a hernia of a mixed nature, forming an intermediate species between a simple and pure ventral hernia, and the common and perfect inguinal hernia.

ANOTHER essential circumstance respects the position of the hernia with regard to the course of the epigastric artery. In ordinary cases of inguinal hernia, the epigastric artery runs on the inside of the hernia, but in this variety it constantly runs on the outside. This leads to some important considerations in practice, though Mr RUSSEL did not enter fully into the applications of the peculiarities, either to practical points, or to the explanation of various curious circumstances in the history of hernial swellings.

ANTIQUITIES.

## ANTIQUITIES.

THE following letter from the Abbé MANN was read 18th June 1799.

1799.  
June 18.  
Concerning the  
Chartreuse of  
Perth.

*To the President and Members of the Royal Society of Edinburgh.*

THE following few notes, concerning the *Chartreuse* of Perth, of no merit but as they regard an exiguous part of the Scottish history, were made in the Austrian Netherlands, from original manuscripts now swallowed up by the revolution of those countries, and for ever lost. That these short extracts may not likewise fall into oblivion in a foreign country, they are offered, with great respect, to the learned Royal Society of Scotland, by their most obedient humble servant,

T. A. MANN, F.R. & A.S. of London.

*Prague in Bohemia, May 1. 1798.*

EXTRACTA MANUSCRIPTORUM *de Carthusiâ Vallis Virtutum, in Suburbano Perthano.*

“ANNO 1430, JACOBUS I. Scotiæ Rex, fundavit Domum Virtutum in suburbano Perthano. OSWALD DE CORDE primus Prior \*.”

THE Scots historians say that King JAMES I. of Scotland founded with great cost and magnificence the Carthusian monastery near Perth, and endowed it with large revenues; and also, that he was buried in the church thereof †.

D 2

“OSWALDUS

\* Ex Collectaneis Manuscriptis PETRI DE WALE, vol. i. p. 9. penes Carth. Bruxelensem.

† So BUCHANNAN. STEWART'S Short Historical and Genealogical Account, p. 64. 65. Scottish Compend. p. 18.

“OSWALDUS DE CORDA, primus Prior in Scotia, fuit imprimis Carthusiæ majoris Vicarius; subindè Domûs Virtutum in Scotia, Prior primus effectus. Fuit acer ingenio, doctrinâ præclarus, vitæ sanctimoniâ eximius. Inter alia multa, condidit pro correctura, librorumque Ordinis Carthusiani correctoribus, opus imprimis utile, multisque acceptum, et junioribus monachis pernecessarium; utpote quod in lectionibus suis eos probè dirigere queat. Postquam autem diutinos in suscepto regimine suscepisset labores, tandem coram positis fratribus ac orantibus, diem clausit extremum, anno Christi tricesimo quarto post mille quadringentos.”

“OBIIT D. OSWALDUS primus Prior Domûs Vallis Virtutum in Scotia, professus domûs Hortus Christi, deinde vicarius Majoris Carthusiæ \*.”

#### ORDINATIO CAPITULI GENERALIS, ANNI M,CCCC,II.

“DOMUS Vallis Virtutum in Scotia, ad instantiam Vicarii et Conventûs, eximitur à provincia Picardiæ remotioris, et provinciâ Gebennensi incorporatur, et sub speciali cura Reverendi Patris (Generalis) ponitur †.”

FROM this time (1442) the Chartreuse of the *Vale of Virtues* near Perth, was annumerated in the province of the *Grande Chartreuse*, or of Geneva, as it was anciently called, after having been annexed for twelve years to the province of the Low Countries.

1442. “MORTUO Adamo Priore, præficitur in priorem domûs Vallis Virtutum, à capitulo generali, Dom. LAURENTIUS HENTON, professus dictæ domûs, non obstante quòd prædictus in ordine Cisterciensi fuit professus, super quo cum illo dispensatur ‡.”

1489.

\* Ex Chartis Capituli Generalis Ordinis Carthus., anni 1435.

† Ex Charta Capituli Generalis, anni 1442.

‡ Ibid. Ex Chartis Capituli Gen. Ord. Cart. 1489.



1489. "OBIIT D. RICH. GAERGEN, monachus professus domûs Axholme, Prior Domûs Vallis Virtutum in Scotia."

1502. "D. ROB. BELLINTON, professus Domûs Vallis Virtutum in Scotia, et singularis benefactor ejus, obiit 21. Septembris. Aliàs fuit abbas Sanctæ Crucis, ordinis SANCTI AUGUSTINI."

1518. "OBIIT ROB. DANSON, professus Domûs Mount-grace; aliàs procurator Vallis Virtutum in Scotia."

1525. "OBIIT ROLAND BROWN, diaconus professus Domûs Scotiæ."

ABOUT this time, ADAM, natural son of King JAMES V. of Scotland, by ELIZABETH, daughter of JOHN, Earl of Lennox, was Prior of the Charter-house at Perth \*.

IN the year 1559, after the preaching of JOHN KNOX, says SPEED †, the Charter-house at Perth, which was the fairest abbey in the realm of Scotland, was destroyed.

1561. "Ultimus Prior *Domûs Virtutum* fuit D. ADAM FORMAN, qui cùm anno 1561, esset procurator Domûs Belnæ, missus fuit cum Patribus GUIL. RUPRE, procuratore domûs Aurai, et ADAMO DESPES, priore Domûs Rosæ, à Capitulo Generali ordinis, ad visitandum desolatam et destructam Domum Vallis Virtutum in Scotia; Priorque factus aliquandiu latitans, domûs negotia prout poterat administrabat, usque ad annum 1562 ‡."

1567. "OBIIT D. JOANNES SPEY, procurator Domûs Scotiæ §."

1590. "OBIIT D. GUIL. CLAPEN, ultimus professus Domûs Vallis Virtutum in Scotia, hospes in domo Vallis profundi ||."

1593. "OBIIT Reverendissimus in Christo Pater D. GUILL. CHERSOLME, Dumblanensis §, professus majoris Carthusiæ (propè Gratianopolim),

\* Stewart's Historical and Genealogical Account; p. 90.

† Speed's History of England, p. 1137.

‡ Ex Collectaniis Petri de Wale, MSS. penes Carthusiam Bruxellensem; vol. iii. p. 185.

|| Ex Chartis Capituli Gen. Ord. Carthusiensis.

§ Ex Collectaneis ejusd. de Wale, vol. iii. p. 24.

Gratianopolim), Prior Carthusiæ Romæ, et procurator generalis ordinis; aliàs Prior Sancti Spiritûs Lugduni, ante ordinis ingressum *Baro*. Sub Scotiæ defectionem, Romam se conferens, atque episcopali dignitate se abdicari, impetrato à Summo Pontifice (qui eum ad altiorem gradum promovere studebat) consensu, ad Majorem Carthusiam contendit, in qua monachus professus ad annos aliquot quàm humillimè vixit, conferens subinde ordines sacros suis confratribus. Missus quoque fuit Legatus ad JACOBUM, Scotiæ regem, quem ex sacro fonte olim susceperat, scribitur rem prudenter fecisse. Fuit aliquandò Episcopus Vassionensis.

*N. B.* The above particulars are all that I have found concerning the Carthusian Order in Scotland. There appears nowhere the least trace or mention of any other Chartreuse having ever existed in that kingdom, besides the above *Vale of Virtues* near Perth.

To the above information by the Abbé MANN, a communication from the Reverend JAMES SCOTT, one of the ministers of Perth, and a learned Scottish antiquary, enables us to add, that the *Chartreuse* of Perth was a very considerable edifice, copied from the Grande Chartreuse in Dauphiny, though on a smaller scale, and situated at the west end of the town, on the spot where the King's Hospital now stands. It therefore occupied a beautiful situation, and one not unworthy of a colony from the fairest seat of monastic retirement. No vestiges of the building now remain.

THE letter of the Prior of the Grande Chartreuse is still extant, addressed to JAMES I. of Scotland, authorising the erection of this religious establishment, and directing that the prior of the Carthusian Monastery of the *Mount of Grace* at Ingleby in Yorkshire,

shire, should send two of his monks to superintend the construction of the building. This letter is dated in 1426. The demolition of the religious houses at Perth began in 1559. ADAM FORMAN, the last prior, and some other of the monks who did not adopt the reformed religion, retired to Errol, not far distant, where they possessed a considerable property. This was probably after the date of 1562, mentioned above.

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ACCOUNT of the term SKULL or SKOLL as used in old Writings, being an Article in the *Etymological Dictionary of the Scottish Language*, proposed to be published by the Reverend JOHN JAMIESON, D. D. Read April 1803.

1803.  
April.  
Explanation of  
the old word  
*skull* or *skoll*.

THE work, of which this communication is to make a part, is intended to illustrate the affinity of the language spoken in the low country of Scotland, with the Saxon, the Islandic, and other dialects of the Gothic. The importance of this undertaking cannot be questioned. Etymology is often the only light by which we can trace the migrations of the early inhabitants of countries, and, in the present instance, may afford the explanation of a leading fact in the history of this island, viz. the establishment of the language in question over all the low part of Scotland, from a period of very remote antiquity, while the *Gaelic* was the language of the mountainous part. The following is an extract from Dr JAMIESON's paper.

SKUL,

“SKUL, SKULL, SKOLL. 1. A goblet or large bowl, for containing liquor of any kind.

The Troiane women stude with hare doun schaik  
About the bere, weping with mony allake :  
And on we keft of warme milk mony a *skul*,  
And of the blude of sacrifice coupis ful :  
The faule we bery in sepulture on this wyse,  
The lattir halefing fyne loud schoutit thrys.

DOUGL. *Virg.* 69. l. 20.

As *coupis* corresponds to *pateras* in the original, *skul* is used for *cymbia*, which DOUGLAS elsewhere renders in this manner ;

Tua silver coppis schapin like ane bote.

*Ibid.* 136. l. 35.

We are not, however, hence to conclude, that the word *skull* necessarily denoted a vessel of this form ; for he elsewhere uses it, conjoined with *flagon*, in rendering *crateras* :

For ioy thay pingil than for till renew  
Thare bankettis with al obseruance dew ;  
And for thir tithingis, in *flakoun* and in *skull*,  
Thay skynk the wyne, and wauchtis cowpis full.

*Ibid.* 210. l. 5.

2. THE term has been metonymically used to denote the salutation of one who is present, or the respect paid to an absent person, by expressing a wish for his health ; while he who does so at the same time partakes of the drink that is used by the company, in token of his cordiality. This is what is now called “drinking one’s health.” In this sense it occurs in the account of Gowrie’s conspiracy, published by royal authority. “The kinge

king called for drinke, and in a merry and homely manner fayde to the Earle, that although the Earle had seen the fashion of entertaynements in other countries, yet hee would teach him the Scottish fashion, seeing he was a Scottish man; and therefore, since he had forgotten to drinke to his Majestie, or sit with his guests and entertayne them, his Majestie would drinke to him his owne welcome, desiring hime to take it forth and drink to the rest of the company, and in his Majesties name to make them welcome.”—

“ When they had near-hand dined, the Earl of GOWRIE came from his Majesties chamber, to drink his *scoll* to my Lord Duke and the rest of the company, which he did. And immediately after the *scoll* had passed about, this deponent raife from the table, to have waited upon his Majesty, conform to his former direction,” &c. p. 196. 227. Perth edit. 1774. In CROMARTY’S edition there is the following note, “ *Scoll*, the word used then for drinking a health.” The passage itself is also differently expressed in this edition. “ The Earl of Gowrie came from his Majesty’s chamber *to the ball, and called for wine, and said that he was directed from his Majesty’s chamber to drink his scoll to my Lord Duke,*” &c. Historical Account, p. 40.

As it is said, that “ GOWRIE came from his Majesties chamber to drink his scoll to my Lord Duke,” it has been supposed that the king desired them to drink his health in his absence. But in what way soever the passage be read, it does not appear that this is the meaning. The relative *his*, might be understood in reference to GOWRIE himself; as intimating that the king desired the Earl to go and welcome the company to his house, by drinking to them. But although it be viewed as referring to JAMES, as it is immediately connected with these words, “ came from his Majesties chamber,” it will not follow, that it was the king’s desire that his own health should be drunk. From what he had previously said to GOWRIE with respect to his omission, it is evi-

dent that this is not the sense of the language. He, in a jocular way, reprehended the Earl for not drinking to him; "desiring him to take it forth, (that is, the drink formerly mentioned) and drink to the rest of the company." Therefore, even admitting that the expression *his scoll* means the *king's scoll*, we cannot with propriety suppose that any thing more is meant, than that GOWRIE went to the antichamber, to convey to the noblemen and gentlemen who were there, his Majesty's salutation; or, as expressed in the narrative, "to drink to the rest of the company, and in his Majesty's name" to give them that welcome, which he had neglected to give them in his own.

THUS it appears, that the term, primarily denoting a vessel for containing liquor, was, in consequence of the customs connected with drinking, at length used to signify the mutual expressions of regard employed by those engaged in computation, or their united wishes for the health and prosperity of one individual, distinguished in rank, or peculiarly endeared to them all, whether he were present or absent.

I HAVE met with one passage in which that expression, the *king's skole*, is distinctly used in the sense which has been improperly attached to the phrase already considered. After the Bridge of Berwick had been rebuilt in the year 1621, "Sir WILLIAM BEYER, mayor of the town, stayed the taking away of the centries, and putting in the keystone, till *the king's skole* were drunk at that part of the bridge." CALDERWOOD's *Hist.* p. 787. But the expression, although equivalent to what is now called drinking the *king's health*, seems strictly to signify, drinking the *king's cup*, or a cup in honour of the king.

For we are not to suppose, that the word *skoll* has any primary or proper relation to health or prosperity. This would be totally repugnant to analogy; as will appear from a comparison of our term with its cognates in the other Northern languages.

ISL.

Isl. *Skal*, *skaal*, *skyldi*, Aleman. *skala*, Germ. *schale*, SuioGoth. and Dan. *skaal*, all signify a cup, a bowl, or drinking vessel. From the Gothic nations, this word seems to have passed to the Celtic. For, in the Cornish, *skala* has the same meaning, being rendered by LHUYD *patera*. RUDDIMAN, in his Glossary to DOUGLAS's *Virgil*, mentions the verb to *skole* or *skolt*, as used Scot. Bor. in the sense of *pocula exinanire*. This verb has undoubtedly been formed from the noun. In the North of Scotland, also, *skiel* still denotes a tub; thus a washing-tub is called a *washing-skiel*. The tubs used by brewers for cooling their wort, are in like manner called *skiels*. It affords a strong presumption that this is originally the same word with *skoll*, *skull*, immediately under consideration, that the goblet employed by the inhabitants of the north for preparing their *ale* for immediate use, is called *kalt-skaal*. This seems to intimate, that our use of the term, with respect to the operation of brewing, contains an allusion to its more ancient appropriation. "*Kaltskaal*, eodem tropo illis quo Sueonibus est *patera*, in qua *frigidus cerevisiæ potus* in æstate, et *calidus* in hieme fieri solet." LOCCENII *Antiq. Sue-Goth.* p. 96.

It may be added, that *skiel* is still used in Orkney as the name of a flaggon, or wooden drinking vessel with a handle.

*Skull* is a term of general use in Scotland for a basket of a semi-circular form. It was used in this sense so early as the time of DUNBAR :

"Fish wyves cry Fy, and cast down *skulls* and *skeills*."

*Evergreen*, ii. p. 59. ft. 23.

It is probable that *skiel* was used by him as if it had been synonymous, because of the alliteration. Or, from the resemblance with respect to form, it may actually have been used in the same sense in his time. Eng. *Skillet*, a small kettle or boiler,

might appear at first view to have some affinity. But it seems immediately formed from Fr. *esculette*, a porringer; and this again from Ital. *scudella*, used in the same sense. This is derived from Lat. *scutula*, which was a kind of concave vessel, a faucer. The learned IHRE, in his *Glossarium SuioGothicum*, views these French, Italian, and Latin words, as allied to Gothic *skaal*. But it is surprising, that he should consider *skaal* itself as formed, *per crasin*, from Lat. *scutula*. The quotations he has himself made for illustrating this word, certainly supplied him with a far more natural etymon. But before proceeding to this, it may be remarked as a singular analogy, that, according to ATHENÆUS, liv. iv. Gr. *σκαλλιον* is a small cup, and *σκαλις* is equivalent to *σκαφιον*, which signifies a drinking-vessel.

It is highly probable, that a cup or bowl received this name from the barbarous custom which prevailed among several ancient nations, of drinking out of the skulls of their enemies. WARNEFRID, in his work *De Gestis Longobard.*, says, ‘ALBIN slew CUNIMUND; and having carried away his head, converted it into a drinking-vessel; which kind of cup is with us called *schala*, but in the Latin language it has the name of *patera*,” lib. i. cap. 27. The same thing is asserted of the Boii by LIVY, lib. xxiii. c. 24.; of the Scythians by HERODOTUS, lib. ix.; of their descendants the Scordisci, by RUFUS FESTUS in *Breviario*; of the Gauls by DIODORUS SICULUS, lib. v.; of the Celts by SILIUS ITALICUS, lib. xiv.

At Celtæ vacui capitis circumdare gaudent,  
Ossa, nefas! auro, et mensis ea pocula servant.

Vide KEYSER. *Antiq. Septentr.* p. 363.

HENCE RAGNAR LODBROG, in his death-song, consoles himself with this reflection; “I shall soon drink beer from the hollow cups made of skulls.” St. 25. WORMII *Literat. Dan.* p. 203.  
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THE same word in SuioG. signifies both a skull (*cranium*), and a drinking-vessel. This observation is equally applicable to Germ. *schale*. But IHRE is so unfavourable to this derivation, principally, as it would appear, from its exhibiting our Gothic ancestors as so extremely barbarous, that he considers the human skull as receiving the name of *skaal* from its resemblance to the *patera* or bowl. This is surely to invert the natural order. Although the Northern nations were greatly addicted to inebriety, yet we can scarcely suppose that they found it necessary to borrow a name for their skulls from their drinking-vessels. The skull itself seems to have received this designation from its resemblance to a *shell*, in Ang. Sax. *sceala*, *scala*, Belg. *schale*, Germ. *schale*, Ill. *skael*, SuioG. and Dan. *skaal*. Allied to this is Moeso-Goth. *skaljos*, the tiling of a roof.

IHRE objects to this etymon, not only on the ground of the inhumanity of the custom supposed to be alluded to; but especially, he says, because he does not find that the word *skaal* is used by ancient writers, as denoting a memorial potation, or the act of drinking in honour of some distinguished personage; adding, that *minne* and *full* are the terms used by old Islandic authors. Even supposing this to be true, it will not disprove the antiquity of the word. Nothing more could reasonably be inferred, than that *skaal*, in more early ages, had retained its original and proper signification, as it is used in the other sense only by a strong metonymy. It was natural to prefer *minne*; for, as literally signifying *memory*, it simply and directly suggested the reason of this particular mode of drinking. Nor need we be surprised, although they even preferred the other term *full*; not only as the figure is less strong, to speak of drinking the *fill* of a cup, than of drinking the cup itself; but also because it referred to the established custom with respect to this draught, that the cup must be *full* and be completely evacuated. This is only to suppose the Islandic word to have been for some  
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time stationary in its meaning, in the same manner with our *skull* or *skoll*. For there is not the slightest evidence that, in the age of GAWIN DOUGLAS, it was used in that figurative sense which it bore a century afterwards.

BUT it is astonishing, that this learned writer, after he has quoted WARNEFRID, should lay any stress on this circumstance, "He does not find that the word *skaal* is used by ancient writers." And can he deny this character to WARNEFRID, who flourished about the year 774? Does not he say that this kind of cup, made of a human skull, is by the Goths called *schale*? Can any Scandinavian writer be produced, who uses *minne* and *full*, to the exclusion of *skaal*, in an earlier age? There is no evidence that either of these terms was written for some ages after. WARNEFRID was not only a writer of great reputation, but himself a Goth; and his positive testimony is surely far preferable to the negative evidence deduced from posterior writers. Although it could be proved, as it cannot, that the term was not used, in that early period, in the particular sense referred to, it would by no means follow, that it was unknown in its simple signification, as denoting a drinking-vessel. As the *Lombardi* were a Gothic nation, it is extremely improbable, that a term which had so singular an origin would be unknown to other nations belonging to the same race; although, without any reason that we can see, it might be more used by one nation than by another.

NOT only is the meaning of this term, as it occurs in other Northern languages, preserved in ours; but the figurative sense is also the same. Thus LOCCENIUS tells us, "Illud nomen in his septentrionalibus locis adhuc ita remanet, ut *dricka skala*, i. e. bibere pateram, metonymicè dicatur, quando bibitur alicujus honori et memoriæ, quòd ex hoc vasculo quondam frequentius fieri fuetum, notio vocis indicat." *Antiq. Sueo-Goth.* p. 96. "In computations," says IHRE, "the name of *skaal* is given to the

the memory of the absent, or the salutation of those who are present, which goes round in the time of drinking;" or more fully, "*dricka ens skaal*." As Dan. *skaal* signifies a bowl or drinking-vessel; *at dricka ens skaal* is to drink one's health. Voc. *skaal*. In ISIDORUS we find the phrase, "Calices et *scalæ*, poculorum genera." *Origin.* lib. xx. c. 5.

IN the same manner did the ancient Goths express their regard to their sovereigns. They drank the *king's skoll*. Hence WARNEFRID relates, that when GRIMAOLD, King of the Lombards, had determined to kill BERTARIDUS, after he was overpowered with wine, the ministers of the palace being ordered to bring to him liquors, with dishes of various kinds, asked of him, in the king's name, to drink a full bowl in honour of him. But he, suspecting the snare, secretly procured that it should be filled with water. Immediately, promising that he would drink it off in honour of the king, he made a libation, by pouring out a little of the water. *De Gestis Longobard.* lib. v. These *skolls* in honour of the king, as we learn from LOCCENIUS, they used also to drink standing. *Ubi supra.*

# THE HISTORY OF THE

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BIOGRAPHICAL ACCOUNT of the late Dr JAMES HUTTON,  
F. R. S. EDIN.

[Read by Mr PLAYFAIR, 10th January 1803.]

DR JAMES HUTTON was the son of Mr WILLIAM HUTTON, merchant in Edinburgh, and was born in that city on the 3d of June 1726. His father, a man highly respected for his good sense and integrity, and who for some years held the office of City Treasurer, died while JAMES was very young. The care of her son's education devolved of course on Mrs HUTTON, who appears to have been well qualified for discharging this double portion of parental duty. She resolved to bestow on him a liberal education, and sent him first to the High School of Edinburgh, and afterwards to the University, where he entered as a student of humanity in November 1740.

OF the masters under whom he studied there, MACLAURIN was by far the most eminent, and Dr HUTTON, though he had cultivated the mathematical sciences less than any other, never mentioned the lectures of that celebrated Professor but in terms of high admiration.

HE used also to acknowledge his obligations to Professor STEVENSON's Prelections on Logic; not so much, however, for having made him a logician as a chemist. The fact that gold is dissolved in *aqua regia*, and that two acids which can each of

them singly dissolve any of the baser metals, must unite their strength before they can attack the most precious, was mentioned by the Professor as an illustration of some general doctrine. The instinct of genius, if I may call it so, enabled Mr HUTTON, young as he then was, to feel, probably, rather than to understand, the importance of this phenomenon; and as if, by the original constitution of his mind, a kind of elective attraction had drawn him towards chemistry, he became from that moment attached to it by a force that could never afterwards be overcome. He made an immediate search for books that might give him some farther instruction concerning the fact which he had just heard of; but the only one he could procure, for a long time, was HARRIS's *Lexicon Technicum*, the predecessor of those voluminous compilations which have since contributed so much more to extend the surface, than to increase the solidity of science. It was from the imperfect sketch contained in that dictionary, that he derived his first knowledge of chemistry, his love for which never forsook him afterwards, and was in truth the propensity which decided the whole course and complexion of his future life.

THOUGH his taste and capacity for instruction were sufficiently conspicuous during his course of academical study, his friends wished him rather to pursue business than science. This was a measure by no means congenial to his mind, yet he acquiesced in it without difficulty.

ACCORDINGLY, in 1743 he was placed as an apprentice with Mr GEORGE CHALMERS, writer to the Signet; and subjection to the routine of a laborious employment, was now about to check the ardour and repress the originality of a mind formed for different pursuits. But happily the force of genius cannot always be controlled by the plans of a narrow and short-sighted prudence. The young man's propensity to study continued, and he was often found amusing himself and his fellow apprentices with

with chemical experiments, when he should have been copying papers, or studying the forms of legal proceedings; so that Mr CHALMERS soon perceived that the business of a writer was not that in which he was destined to succeed. With much good sense and kindness, therefore, he advised him to think of some employment better suited to his turn of mind, and released him from the obligations which he had come under as his apprentice. In this he did an essential service to science, and to the young man himself. A man of talents may follow any profession with advantage; a man of genius will hardly succeed but in that which nature has pointed out.

THE study of medicine, as being the most nearly allied to chemistry, was that to which young HUTTON now resolved to dedicate his time. He began that study under Dr GEORGE YOUNG, the father of the late Dr THOMAS YOUNG, and at the same time attended the lectures in the University. This course of medical instruction he followed from 1744 to 1747.

THOUGH a regular school of medicine had now been established in the University of Edinburgh for several years, the system of medical education was neither in reality, nor in the opinion of the world, so complete as it has since become. Some part of a physician's studies was still to be prosecuted on the Continent; and accordingly, in the end of 1747, Mr HUTTON repaired to Paris, where he pursued with great ardour the studies of chemistry and anatomy. After remaining in that metropolis nearly two years, he returned by the way of the Low Countries, and took the degree of Doctor of Medicine at Leyden in September 1749. His thesis is entitled, *De Sanguine et Circulatione in Microcosmo*.

ON his return to London about the end of that year, he began to think seriously of settling in the world. His native city, to which his views of course were first turned, afforded no very flattering prospect for his establishment as a physician. The

business there was in the hands of a few eminent practitioners who had been long established; so that no opening was left for a young man whose merit was yet unknown, who had no powerful connections to assist him on his first outset, and very little of that patient and circumspect activity by which a man pushes himself forward in the world.

THESE considerations seem to have made a very deep impression on his mind, and he wrote on the subject of his future prospects with considerable anxiety to his friends in Edinburgh.

ONE of these friends was Mr JAMES DAVIE, a young man nearly of his own age, with whom he had early contracted a very intimate friendship, that endured through the whole of his life, without interruption, to the mutual benefit of both. The turn which both of them had for chemical experiments formed their first connection, and cemented it afterwards. They had begun together to make experiments on the nature and production of sal ammoniac. These experiments had led to some valuable discoveries, and had been farther pursued by Mr DAVIE during Dr HUTTON's absence. The result afforded a reasonable expectation of establishing a profitable manufacture of the salt just named from coal-foot.

THE project of this establishment was communicated by Mr DAVIE to his friend, who was still in London, and it appears to have lessened his anxiety about settling as a physician, and probably was one of the main causes of his laying aside all thoughts of that profession. Perhaps, too, on a nearer view, he did not find that the practice of medicine would afford him that leisure for pursuing chemical and other scientific objects, which he fancied it would do when he saw things at a greater distance. Whatever was the cause, it is certain that soon after his return to Edinburgh in summer 1750, he abandoned entirely his views of the practice of medicine, and resolved to apply himself to agriculture.

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THE motives which determined him in the choice of the latter, cannot now be traced with certainty. He inherited from his father a small property in Berwickshire, and this might suggest to him the business of husbandry. But we ought rather, I think, to look for the motives that influenced him, in the simplicity of his character, and the moderation of his views, than in external circumstances. To one who, in the maturity of understanding, has leisure to look round on the various employments which exercise the skill and industry of man, if his mind is independent and unambitious, and if he has no sacrifice to make to vanity or avarice, the profession of a farmer may seem fairly entitled to a preference above all others. This was exactly the case of Dr HUTTON, and he appears to have been confirmed in his choice by the acquaintance which he made about that time with Sir JOHN HALL of Dunglass, a gentleman of the same county, a man of ingenuity and taste for science, and also much conversant with the management of country affairs.

As he was never disposed to do any thing by halves, he determined to study rural-economy in the school which was then reckoned the best, and in the manner which is undoubtedly the most effectual. He went into Norfolk, and fixed his residence for some time in that country, living in the house of a farmer, who served both for his landlord and his instructor. This he did in 1752; and many years afterwards I have often heard him mention, with great respect, the name of JOHN DYBOLD, at whose house he had lived with much comfort, and whose practical lessons in husbandry he highly valued. He appears, indeed, to have enjoyed this situation very much: the simple and plain character of the society with which he mingled, suited well with his own, and the peasants of Norfolk would find nothing in the stranger to set them at a distance from him, or to make them treat him with reserve. It was always true of Dr HUTTON,

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TON, that to an ordinary man he appeared to be an ordinary man, possessing a little more spirit and liveliness, perhaps, than it is usual to meet with. These circumstances made his residence in Norfolk greatly to his mind, and there was accordingly no period of his life to which he more frequently alluded, in conversation with his friends; often describing, with singular vivacity, the rural sports and little adventures, which, in the intervals of labour, formed the amusement of their society.

WHILE his head-quarters were thus established in Norfolk, he made many journeys on foot into different parts of England; and though the main object of these was to obtain information in agriculture, yet it was in the course of them that to amuse himself on the road, he first began to study mineralogy or geology. In a letter to Sir JOHN HALL, he says that he was become very fond of studying the surface of the earth, and was looking with anxious curiosity into every pit, or ditch, or bed of a river that fell in his way; "and that if he did not always avoid the fate of THALES, his misfortune was certainly not owing to the same cause." This letter is from Yarmouth; it has no date, but it is plain from circumstances, that it must have been written in 1753.

WHAT he learned in Norfolk made him desirous of visiting Flanders, the country in Europe where good husbandry is of the oldest date. He accordingly set out on a tour in that country, early in spring 1754, and travelling from Rotterdam through Holland, Brabant, Flanders, and Picardy, he returned to England about the middle of summer. He appears to have been highly delighted with the garden culture which he found to prevail in Holland and Flanders, but not so as to undervalue what he had learnt in England. He says in a letter to Sir JOHN HALL, written soon after his arrival in London, "Had I doubted of it before I set out, I should have returned fully convinced that they are good husbandmen in Norfolk."

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THOUGH his principal object in this excursion was to acquire information in the practice of husbandry, he appears to have bestowed a good deal of attention on the mineralogy of the countries through which he passed, and has taken notice in his *Theory of the Earth* of several of the observations which he made at that time.

ABOUT the end of the summer he returned to Scotland, and hesitated a while in the choice of a situation where he might best carry into effect his plans of agricultural improvement. At last he fixed on his own farm in Berwickshire, and accordingly set about bringing it into order with great vigour and effect. A ploughman whom he brought from Norfolk set the first example of good tillage which had been seen in that district, and Dr HUTTON has the credit of being one of those who introduced the new husbandry into a country where it has since made more rapid advances than in any other part of Great Britain.

FROM this time till about the year 1768, he resided for the most part on his farm, visiting Edinburgh, however, occasionally. The tranquillity of rural life affords few materials for biographical description; and an excursion to the North of Scotland, which he made in 1764, is one of the few incidents which mark an interval of fourteen years, passed mostly in the retirement of the country. He made this tour in company with Commissioner afterwards Sir GEORGE CLERK, a gentleman distinguished for his abilities and worth, with whom Dr HUTTON had the happiness to live in habits of the most intimate friendship. They set out by the way of Crieff, Dalwhinnie, Fort Augustus, and Inverness; from thence they proceeded through East-Ross into Caithness, and returned along the coast by Aberdeen to Edinburgh. In this journey Dr HUTTON's chief object was mineralogy, or rather geology, which he was now studying with great attention.

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FOR several years before this period, Dr HUTTON was concerned in the sal-ammoniac work, which had been actually established on the foundation of the experiments already mentioned, but remained in Mr DAVIE's name, only, till 1765: at that time a copartnership was regularly entered into, and the work carried on afterwards in the name of both.

HE now found that his farm was brought into the regular order which good husbandry requires, and that as the management of it became more easy, it grew less interesting. An occasion offering of letting it to advantage, he availed himself of it. About the year 1768 he left Berwickshire entirely, and became resident in Edinburgh, giving his undivided attention from that time to scientific pursuits.

AMONG other advantages which resulted to him from this change of residence, we must reckon that of being able to enjoy, with less interruption, the society of his literary friends, among whom were Dr BLACK, Mr RUSSEL, professor of Natural Philosophy, Professor ADAM FERGUSON, Sir GEORGE CLERK, already mentioned, his brother Mr CLERK of Elden, Dr JAMES LIND, now of Windsor, and several others. Employed in maturing his views, and studying nature with unwearied application, he now passed his time most usefully and agreeably to himself, but in silence and obscurity with respect to the world. He was, perhaps, in the most enviable situation in which a man of science can be placed. He was in the midst of a literary society of men of the first abilities, to all of whom he was peculiarly acceptable, as bringing along with him a vast fund of information and originality, combined with that gayety and animation which so rarely accompany the profounder attainments of science. Free from the interruption of professional avocations, he enjoyed the entire command of his own time, and had sufficient energy of mind to afford himself continual occupation.

A GOOD deal of his leisure was now employed in the prosecution of chemical experiments. In one of these experiments, which he has nowhere mentioned himself, but which I have heard of from Dr BLACK, he discovered that mineral alkali is contained in zeolite. On boiling the gelatinous substance obtained from combining that fossil with muriatic acid, he found that, after evaporation, sea-salt was formed. Dr BLACK did not recollect exactly the date of this experiment, but from circumstances judged that it was earlier than 1772: It is, if I mistake not, the first instance of an alkali being discovered in a stony body. The experiments of M. KLAPROTH and Dr KENNEDY have confirmed this conclusion, and led to others of the same kind.

IN 1774 he made a tour through part of England and Wales, of which, I find no memorandum whatever among his papers. I know, however, that at this time he visited the salt-mines in Cheshire, and made the curious observation of the concentric circles marked on the roof of these mines, to which he has referred in his *Theory of the Earth*, as affording a proof that the salt rock was not formed from mere aqueous deposition. His friend Mr WATT of Birmingham accompanied him in his visit to the mines.

It was after returning to Birmingham from Cheshire, that he set out on the tour into Wales. One of the objects of this tour, as I learnt from himself, was to discover the origin of the hard gravel of granulated quartz, which is found in such vast abundance in the soil about Birmingham, and indeed over a great tract of the central part of England. This gravel is so unlike that which belongs to a country of secondary formation, that it very much excited his curiosity; and his present journey was undertaken with a view to find out whether among the primitive mountains of Wales, there were any that might be supposed to have furnished the materials of it. In Wales, however,

he saw none that could, with any probability, be supposed to have done so; and he was equally unsuccessful in all the other parts he visited, till returning, at a small distance from Birmingham, the place from whence he had set out, he found a rock of the very kind which he had been in search of. It belongs to a body of strata apparently primary, which break out between Broomsgrove and Birmingham, and have all the characters of the indurated gravel in question. If, however, they have furnished the materials of that gravel, it seems probable that it has been through the medium of the red sand-stone, which abounds in those countries \*.

IN 1777 Dr HUTTON's first publication was given to the world, viz. a small pamphlet, intituled, *Considerations on the Nature, Quality, and Distinctions of Coal and Culm*. This little work, an octavo pamphlet of 37 pages, was occasioned by a question that had arisen, Whether the small coal of Scotland is the same with the culm of England? and, Whether of course, like the latter, it is entitled, when carried coastwise, to an exemption from the duty on coal? Some of the small coal from the Frith of Forth, which had been carried to the northern counties for the purpose of burning lime, had been considered by the revenue officers as liable to the same duty with other coal, while the proprietors contended that it ought only to pay the lighter duty levied on culm. This was warmly disputed; and after occupying for some time the attention of the Board of Customs in Scotland, was at last brought before the Privy Council.

DR HUTTON's pamphlet was intended to supply the information necessary for forming a judgment on this question. It is very ingenious and satisfactory, though perhaps, considering the purpose for which it was written, it is on too scientific a plan, and

\* Illustrations of the Huttonian Theory, p. 375.

and conducted too strictly according to the rules of philosophical analysis. It proves that culm is the small, or refuse, of the infusible, or stone coal, such as that of Scotland for the most part is; that the small of the fusible coal, by caking or uniting together, becomes equally serviceable with the large coal; whereas the small of the infusible, by running down like loose sand, cannot be made to burn in the ordinary way, and is useful but for few purposes, so that it has been properly exempted from the usual duty on coal. A criterion is also pointed out for determining when small coal is to be regarded as culm, and when it may be considered as coal;—if, when a handful of it is thrown into a red-hot shovel, the pieces burn without melting down or running together, it decidedly belongs to the former\*.

IN the conclusion, an exemption from duty was obtained for the small coal of Scotland, when carried coastwise, and this regulation was owing in a great degree to the satisfactory information contained in Dr HUTTON's pamphlet. It was a step, also, toward the entire abolition of those injudicious duties which had been so long levied on coal, when carried by sea beyond a certain distance from its native place. This abolition happened several years after the period we are speaking of, much to the benefit of the country, and to the credit of the administration under which it took place.

As Dr HUTTON always took a warm interest in whatever concerned the advancement of the arts, particularly in his native country, he entered with great zeal into the project of an internal navigation between the Friths of Forth and Clyde. The comparative merit of the different plans, according to which that work was to be executed, gave rise to a good deal of discussion, and even of controversy. In these debates Dr HUTTON

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\* A few copies of the *Considerations on Culm* are still to be found in the shop of C. ELLIOT, Edinburgh.

took a share, and wrote several pieces, in which the grave and the ludicrous were both occasionally employed. None of these pieces have been published; but the plan that was in the end adopted was that in favour of which they were written. It is unnecessary, however, to enter into the merits of a question which has long ceased to interest the public.

FROM the time of fixing his residence in Edinburgh, Dr HUTTON had been a member of the Philosophical Society, known to the world by the three volumes of physical and literary essays so much and so justly esteemed \*. In that society he read several papers; but it was during the time that elapsed between the publication of the last of the volumes just mentioned, and the incorporation of the Philosophical into the Royal Society of Edinburgh; which last was established by a royal charter in 1783. None of these papers have been published, except one in the second volume of the *Transactions of the Royal Society*, "*On certain Natural Appearances of the Ground on the Hill of Arthur's Seat.*"

THE institution of the Royal Society of Edinburgh had the good effect of calling forth from Dr HUTTON the first sketch of a theory of the earth, the formation of which had been the great object of his life. From the date formerly mentioned, when he was yet a very young man, and making excursions on foot through the different counties of England, till that which we are now arrived at, a period of about thirty years, he had never ceased to study the natural history of the globe, with a view of ascertain-  
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\* THE Philosophical Society was instituted about the year 1739. The first volume of *Essays* was published in 1754; the second in 1756; the third in 1771. From the year 1777 to 1782, the meetings of the Society were pretty regular, much owing to the zeal of Lord KAMES. Mr MACLAURIN may be regarded as the founder of this Society.



ing the changes that have taken place on its surface, and of discovering the causes by which they have been produced.

HE had become a skilful mineralogist, and had examined the great facts of geology with his own eyes, and with the most careful and scrupulous observation. In the course of these studies he had brought together a considerable collection of minerals peculiarly calculated to illustrate the changes which fossil bodies have undergone. He had also carefully perused almost every book of travels from which any thing was to be learned concerning the natural history of the earth; and, in consequence both of reading and observation, was eminently skilled in physical geography.

IF to all this it be added, that Dr HUTTON was a good chemist, and possessed abilities excellently adapted to philosophical research, it will be acknowledged, that few men have entered with better preparation on the arduous task of investigating the true theory of the earth. Several years before the time I am now speaking of, he had completed the great outline of his system, but had communicated it to very few; I believe to none but his friends Dr BLACK and Mr CLERK of Eldon. Though fortified in his opinion by their agreement with him, (and it was the agreement of men eminently qualified to judge), yet he was in no haste to publish his theory; for he was one of those who are much more delighted with the contemplation of truth, than with the praise of having discovered it. It might therefore have been a long time before he had given any thing on this subject to the public, had not his zeal for supporting a recent institution which he thought of importance to the progress of science in his own country induced him to come forward, and to communicate to the Royal Society a concise account of his theory of the earth.

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As I have treated of this theory in a separate Essay, particularly destined to the illustration of it, I shall here content myself with a very general outline.

I. The object of Dr HUTTON was not, like that of most other theorists, to explain the first origin of things. He was too well skilled in the rules of sound philosophy for such an attempt; and he accordingly confined his speculations to those changes which terrestrial bodies have undergone since the establishment of the present order, in as far as distinct marks of such changes are now to be discovered.

WITH this view, the first general fact which he has remarked is, that by far the greater part of the bodies which compose the exterior crust of our globe, bear the marks of being formed out of the materials of mineral or organized bodies, of more ancient date. The spoils or the wreck of an older world are every where visible in the present, and, though not found in every piece of rock, they are diffused so generally as to leave no doubt that the strata which now compose our continents are all formed out of strata more ancient than themselves.

II. The present rocks, with the exceptions of such as are not stratified, having all existed in the form of loose materials collected at the bottom of the sea, must have been consolidated and converted into stone by virtue of some very powerful and general agent. The consolidating cause which he points out is subterraneous heat, and he has removed the objections to this hypothesis by the introduction of a principle new and peculiar to himself. This principle is the compression which must have prevailed in that region where the consolidation of mineral substances was accomplished. Under the weight of a superincumbent ocean, heat, however intense, might be unable to volatilize any part of those substances which, at the surface, and under the lighter pressure of our atmosphere, it can entirely consume. The same pressure, by forcing those substances to remain united,

ted, which at the surface are easily separated, might occasion the fusion of some bodies which in our fires are only calcined. Hence the objections that are so strong and unanswerable, when opposed to the theory of volcanic fire, as usually laid down, have no force at all against Dr HUTTON's theory; and hence we are to consider this theory as hardly less distinguished from the hypothesis of the Vulcanists, in the usual sense of that appellation, than it is from that of the Neptunists, or the disciples of WERNER.

III. THE third general fact on which this theory is founded, is, that the stratified rocks, instead of being either horizontal, or nearly so, as they no doubt were originally, are now found possessing all degrees of elevation, and some of them even perpendicular to the horizon; to which we must add, that those strata which were once at the bottom of the sea are now raised up, many of them, several thousand feet above its surface. From this, as well as from the inflexions, the breaking and separation of the strata, it is inferred, that they have been raised up by the action of some expansive force placed under them. This force, which has burst in pieces the solid pavement on which the ocean rests, and has raised up rocks from the bottom of the sea, into mountains 15,000 feet above its surface, exceeds any which we see actually exerted, but seems to come nearer to the cause of the volcano or the earthquake than to any other, of which the effects are directly observed. The immense disturbance, therefore, of the strata, is in this theory ascribed to heat acting with an expansive power, and elevating those rocks which it had before consolidated.

IV. AMONG the marks of disturbance in which the mineral kingdom abounds, those great breaches among rocks, which are filled with materials different from the rock on either side, are among the most conspicuous. These are the veins, and comprehend, not only the metallic veins, but also those of whinstone,

stone, of porphyry, and of granite, all of them substances more or less crystallized, and none of them containing the remains of organized bodies. These are of posterior formation to the strata which they intersect, and in general also they carry with them the marks of the violence with which they have come into their place, and of the disturbance which they have produced on the rocks already formed. The materials of all these veins Dr HUTTON concludes to have been melted by subterraneous heat, and, while in fusion, injected among the fissures and openings of rocks already formed, but thus disturbed, and moved from their original place.

THIS conclusion he extends to all the masses of whinstone, porphyry, and granite, which are interposed among strata, or raised up in pyramids, as they often appear to be, through the midst of them. Thus, in the fusion and injection of the unstratified rocks, we have the third and last of the great operations which subterraneous heat has performed on mineral substances.

V. FROM this Dr HUTTON proceeds to consider the changes to which mineral bodies are subject when raised into the atmosphere. Here he finds, without any exception, that they are all going to decay; that from the shore of the sea to the top of the mountain, from the softest clay to the hardest quartz, all are wasting and undergoing a separation of their parts. The bodies thus resolved into their elements, whether chemical or mechanical, are carried down by the rivers to the sea, and are there deposited. Nothing is exempted from this general law: among the highest mountains and the hardest rocks, its effects are most clearly discerned; and it is on the objects which appear the most durable and fixed, that the characters of revolution are most deeply imprinted.

ON comparing the first and the last of the propositions just enumerated, it is impossible not to perceive that they are two steps

steps of the same progression, and that mineral substances are alternately dissolved and renewed. These vicissitudes may have been often repeated; and there are not wanting remains among mineral bodies, that lead us back to continents from which the present are the third in succession. Here, then, we have a series of great natural revolutions in the condition of the earth's surface, of which, as the author of this theory has remarked, we neither see the beginning nor the end; and this circumstance accords well with what is known concerning other parts of the economy of the world. In the continuation of the different species of animals and vegetables that inhabit the earth, we discern neither a beginning nor an end; and in the planetary motions, where geometry has carried the eye so far both into the future and the past, we discover no mark either of the commencement or termination of the present order. It is unreasonable, indeed, to suppose that such marks should any where exist. The Author of nature has not given laws to the universe, which, like the institutions of men, carry in themselves the elements of their own destruction; he has not permitted in his works any symptom of infancy or of old age, or any sign by which we may estimate either their future or their past duration. He may put an end, as he no doubt gave a beginning, to the present system, at some determinate period of time; but we may rest assured, that this great catastrophe will not be brought about by the laws now existing, and that it is not indicated by any thing which we perceive.

It would be desirable to trace the progress of an author's mind in the formation of a system where so many new and enlarged views of nature occur, and where so much originality is displayed. On this subject, however, Dr HUTTON's papers do not afford so much information as might be wished for, though something may be learnt from a few sketches of an *Essay on the Natural History of the Earth*, evidently written at a very early period, and intended, it would seem, for parts of an extensive

work, of which, as often happens with the first attempts to generalize, the plan was never executed, and may never have been accurately digested.

FROM these sketches it appears that the first of the propositions just enumerated, viz. that a vast proportion of the present rocks is composed of materials afforded by the destruction of bodies, animal, vegetable, and mineral, of more ancient formation; was the first conclusion that he drew from his observations.

THE second seems to have been, that all the present rocks are without exception going to decay, and their materials descending into the ocean. These two propositions, which are the extreme points, as it were, of his system, appear, as to the order in which they became known, to have preceded all the rest. They were neither of them, even at that time, entirely new propositions, though, in the conduct of the investigation, and in the use made of them, a great deal of originality was displayed. The comparison of them naturally suggested to a mind not fettered by prejudice, nor swayed by authority, that they are two steps of the same progression; and that, as the present continents are composed from the waste of more ancient land, so, from the destruction of them, future continents may be destined to arise. Dr HUTTON accordingly, in the notes to which I allude, insists much on the perfect agreement of the structure of the beds of grit or sandstone, with that of the banks of unconsolidated sand now formed on our shores, and shews that these bodies differ from one another in nothing but their compactness and induration.

IN generalizing these appearances, he proceeded a step farther, considering this succession of continents as not confined to one or two examples, but as indefinitely extended, and the consequence of laws perpetually acting. Thus he arrived at the new and sublime conclusion, which represents nature as having provided for a constant succession of land on the surface of the earth, according

according to a plan having no natural termination, but calculated to endure as long as those beneficent purposes, for which the whole is destined, shall continue to exist.

THIS conclusion, however, was but a suggestion, till the mechanism was inquired into by which this grand renovation may be brought about, or by which loose materials can be converted into stone, and elevated into land. This led to an investigation of the mineralizing principle, or the cause of the consolidation of mineral bodies: And Dr HUTTON appears accordingly, with great impartiality, and with no physical hypothesis whatever in his mind, to have begun with inquiring into the nature of the fluidity which so many mineral substances seem to have possessed previous to the acquisition of their present form. After a long and minute examination, he came to the conclusion, That the fluidity of these substances has been what he terms SIMPLE, that is to say, not such as is produced by combination with a solvent. The two general facts from which this conclusion follows, are, first, that no solvent is capable of holding in solution all mineral substances, nor even all such varieties of them as are often united in the same specimen; and, secondly, that in the bodies composed of fragments of other bodies, the consolidation is so complete that no room is left for a solvent to have ever occupied. The substance, therefore, which was the cause of the fluidity of mineral bodies, and prepared them for consolidation, must have been one that could act on them all, which occupied no space within them, and could find its way through them, whatever was the degree of their compactness and induration. Heat is the only substance which has these properties; and is the only one, therefore, which, without manifest contradiction, can be assigned as the cause of mineral consolidation.

MANY difficulties, however, were still to be removed before this hypothesis was rendered completely satisfactory; but in what order Dr HUTTON proceeded to remove them, the notes above

mentioned do not enable me to state. We may nevertheless conjecture, with considerable probability, what the step was which immediately followed.

It must have occurred to him, as an objection to the consolidation of minerals by subterraneous heat, that many substances are found in the bowels of the earth in a state altogether unlike that into which they are brought by the action of our fires at the surface. Coal, for instance, by exposure to fire, has its parts dissipated; the ashes which remain behind are a substance quite different from the coal itself; and hence it would seem that this fossil can never before have been subjected to the action of fire. But is it certain, (we may suppose Dr HUTTON to have said to himself), if the heat had been applied to the coal in the interior of the earth, at the bottom of the sea, for example, that the same dissipation of the parts would have taken place? Would not the greater compression that must prevail in that region have prevented the dissipation, at least till a more intense heat was applied? And if the dissipation was prevented, might not the mass, after cooling, be very different from any thing that can be obtained by burning at the surface of the earth? It is plain that there is no reason whatever for answering these questions in the negative. And, on the contrary, if the analogy of nature is consulted, if the fact of water requiring more heat to make it boil when it is more compressed, or the experiments with PAPIN's digester, be considered, it will appear that the answer must be in the affirmative. Nay, it could not but seem reasonable to proceed a step farther, and, as the mixture of substances is known in so many instances to promote their fusibility, to suppose that when the volatile parts of bodies were restrained, the whole mass might be reduced into fusion by heat, though, when these same parts were driven off, the residuum might be altogether infusible. Thus coal, when the charcoal and bitumen are forced to remain in union, may very well be a fusible substance, though,



though, when the latter is permitted to escape, the former becomes one of the most refractory of all bodies.

IN this way, and probably from this very instance, the effects of compression may have suggested themselves to Dr HUTTON. He would soon perceive that the same principle could be very generally applied, and that it afforded the solution of a difficulty concerning limestone, similar to that which has been just stated with respect to coal. Limestone is not found in the bowels of the earth having the causticity which it acquires by the action of fire, and hence one might conclude that it had never been exposed to the action of that element. But the experiments of Dr BLACK, before his friend was engaged in this geological investigation \*, had proved that the causticity of lime depends on the expulsion of the æriform fluid, since distinguished by the name of carbonic gas, which composes no less than two-fifths of the whole. This great discovery, which has extended its influence so widely over the science of chemistry, also led to important consequences in geology; and Dr HUTTON inferred from it, that strong compression might prevent the causticity of lime, by confining the carbonic gas, even when great heat was applied, and that, as has been supposed of coal, the whole may have been melted in the interior of the earth, so as on cooling to acquire that crystallized or sparry structure which the carbonate of lime so frequently possesses †.

It

\* Dr BLACK's paper on magnesia, which contained this discovery, was communicated to the Philosophical Society of Edinburgh in June 1755, and was published in the second volume of their Essays, in the year following. Dr HUTTON had at this time only begun his geological researches. It was not, I imagine, till after the year 1760 that they came to take the form of a theory.

† In the view here presented of the principle of compression, as employed in the Huttonian Theory, it is considered as a hypothesis, conformable to analogy, assumed for the purpose of explaining certain phenomena in the natural history of the

It is unnecessary to carry our conjectures concerning the train of Dr HUTTON's discoveries to a greater length; the development of the principles now enumerated, and the comparison of the results with the facts observed in the natural history of minerals, led to those discoveries, by a road that will be easily traced by those who study his theory with attention.

It might have been expected, when a work of so much originality as this Theory of the Earth, was given to the world, a theory which professed to be the result of such an ample and accurate

the earth. It rests, therefore, as to its evidence, partly on its conformity to analogy, and partly on the explanation which it affords of the phenomena alluded to. In supposing that it derives probability from the last-mentioned source, we are far from assuming any thing unprecedented in sound philosophy. A principle is often admitted in physics, merely because it explains a great number of appearances; and the theory of GRAVITATION itself rests on no other foundation.

THE degree of this evidence will perhaps be differently appreciated, according to a man's habits of thinking, or the class of studies in which he has been chiefly engaged. To Dr HUTTON himself it appeared very strong; for he considered the fact of the liquefaction of mineral substances by heat as so completely established, that it affords a full proof of the fusibility of those substances having been increased by the compression which they endured in the bowels of the earth. In his view of the matter, no other proof seemed necessary, and he did not appear to think that the direct testimony of experiment, could it have been obtained, would have added much to the credibility of this part of his system.

For my part, I will acknowledge that the matter appears to me in a light somewhat different, and that though the arguments just mentioned are sufficient to produce a very strong conviction, it is a conviction that would be strengthened by an agreement with the results even of such experiments as it is within our reach to make. It seems to me, that it is with this principle in geology, much as it is with the parallax of the earth's orbit in astronomy; the discovery of which, though not necessary to prove the truth of the COPERNICAN SYSTEM, would be a most pleasing and beautiful addition to the evidence by which it is supported. So, in the Huttonian geology, though the effects ascribed to compression, are fairly deducible from the phenomena of the mineral kingdom itself, compared with certain analogies which science has established, yet the testimony of direct experiment would make the evidence complete, and would leave nothing that incredulity itself could possibly desiderate.

accurate induction, and which opened up so many views, interesting not to mineralogy alone, but to philosophy in general, that it would have produced a sudden and visible effect, and that men of science would have been every where eager to decide concerning its real value. Yet the truth is, that it drew their attention very slowly, so that several years elapsed before any one shewed himself publicly concerned about it, either as an enemy or a friend.

SEVERAL causes probably contributed to produce this indifference. The world was tired out with unsuccessful attempts to form geological theories, by men often but ill informed of the phenomena which they proposed to explain, and who proceeded also on the supposition that they could give an account of the *origin* of things, or the first establishment of that system which is now the order of nature. Men who guided their inquiries by a principle so inconsistent with the limits of the human faculties, could never bring their speculations to a satisfactory conclusion, and the world readily enough perceived their failure, without taking the trouble to inquire into the cause of it.

TRUTH, however, forces me to add, that other reasons certainly contributed not a little to prevent Dr HUTTON's theory from making a due impression on the world. It was proposed too briefly, and with too little detail of facts, for a system which involved so much that was new, and opposite to the opinions generally received. The descriptions which it contains of the phenomena of geology, suppose in the reader too great a knowledge of the things described. The reasoning is sometimes embarrassed by the care taken to render it strictly logical; and the transitions, from the author's peculiar notions of arrangement, are often unexpected and abrupt. These defects run more or less through all Dr HUTTON's writings, and produce a degree of obscurity astonishing to those who knew him, and who heard him.

him every day converse with no less clearness and precision, than animation and force. From whatever causes the want of perspicuity in his writings proceeded, perplexity of thought was not among the number; and the confusion of his ideas can neither be urged as an apology for himself, nor as a consolation to his readers.

ANOTHER paper from his pen, a *Theory of Rain*, appeared also in the first volume of the *Edinburgh Transactions*. He had long studied meteorology with great attention; and this communication contains one of the few speculations in that branch of knowledge entitled to the name of *theory*.

DR HUTTON begins with supposing that the quantity of humidity, which air is capable of dissolving, increases with its temperature. Now, this increase must either be in the same ratio with the increase of heat, in a less ratio, or in a greater: in other words, for equal increments of heat, the increments of humidity must either constitute a series of which all the terms are equal to one another, or a series in which the terms continually decrease, or one in which they continually increase \*. If either of the two first laws was that which took place in nature, a mixture of two portions of air, though each contained as much humidity as it was capable of dissolving, would never produce a condensation of that humidity. According to the first

\* To speak strictly, the law which connects the increments of humidity in the air with the increments of temperature, is not confined to any one of the three suppositions here made, but may involve them all. The humidity dissolved may be proportional to some *function* of the heat, that varies in some places faster, and in others slower, than in the simple ratio of the heat itself. Nevertheless, for that extent to which observation reaches, the reasoning of Dr HUTTON is quite sufficient to prove that it varies faster; or, in other words, that if a curve be supposed, of which the abscissæ represent the temperature, and the ordinates the humidity, this curve, though it may in the course of its indefinite extent be in some places concave and in others convex toward the axis, is wholly convex in all that part with which our observations are concerned.

first law, the temperature, the humidity, and the power of containing humidity, in the mixture, being all arithmetical means between the same quantities, as they existed previously to the mixture, the temperature produced would be exactly that which was required by the humidity to preserve it in its invisible form. If the second law took place, the moisture actually contained in the mixture would be less than the temperature was capable of supporting; so that instead of a condensation of humidity, the air would become drier than before.

If, on the other hand, the third law be that which takes place, after the mixture of two portions of air of different temperatures, the humidity will be greater than the temperature is able to maintain, and therefore a condensation of it will follow. Now, the experience of every day proves, that the mixture of two portions of humid air of unequal temperatures, does indeed produce a condensation of moisture, and therefore we are authorised to conclude that the last-mentioned law is that which actually prevails\*.

It is obvious that this principle affords an explanation of the formation of clouds in the atmosphere, and that currents of air,

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\* It has been supposed that the chemical solution of humidity in air is necessarily implied in this theory of rain. The truth is, that the air is here considered only as the vehicle of the vapour, and that the transparent state of the latter is supposed to depend on the temperature, or the quantity of heat; but whether that heat act on the vapour solely and directly, or indirectly, by increasing the power of the air to retain it in solution, is, with respect to this theory, altogether immaterial.

Dr HUTTON has indeed used the common language concerning the solution of humidity in air; but the supposition of such solution is not essential to his theory. He seemed, indeed, to entertain doubts about the reality of that operation, founded on the circumstance of evaporation taking place *in vacuo*. Experiments made by M. DALTON since the death of Dr HUTTON, shew that there is great reason for supposing that the air has no chemical action whatever on the aqueous vapour contained in it. *Manchester Memoirs*, vol. v. p. 538.

or winds, of different temperatures, when they meet, must produce such mixtures as have been described, and give rise consequently to the condensation of aqueous vapour. When the supply of the humid air, entering into the mixture, is continued, the quantity of cloud formed will continually increase, and the small globules of condensed moisture, uniting into drops, must descend in rain.

BUT though we are thus in possession of a principle by which rain may be certainly produced, yet whether it be the only one by which rain is produced may require some farther investigation. Dr HUTTON accordingly, in order to determine this point, has entered into a very ample detail concerning the rain under different climates, and in different regions of the earth. The result is, that the quantity of rain is, as nearly as can be estimated, every where proportional to the humidity contained in the air, and the causes which promote the mixture of different portions of air, in the upper regions of the atmosphere. Between the tropics, for instance, the dry season is that in which the uniform current of the trade-wind meets with no obstruction in its circuit round the globe; and the rainy season happens when the sun approaches to the zenith, and when the steadiness of the trade-wind either yields to irregular variations, or to the stated changes of the monsoons.

THUS too, (to mention another extreme case), in certain countries distant from the sea, having little inequality of surface, and exposed to great heat, no rain whatever falls, and the sands of the desert are condemned to perpetual sterility. Even there, however, where a mountainous tract occurs, the mixture of different portions of air produces a deposition of humidity; perennial springs are found; and the fertile vales of Fezzan or Palmyra are exempted from the desolation of the surrounding wilderness.

THIS ingenious theory attracted immediate attention, and was valued for affording a distinct notion of the manner in which

which cold acts in causing a precipitation of humidity. It met, however, from M. DE LUC with a very vigorous and determined opposition; Dr HUTTON defended it with some warmth, and the controversy was carried on with more sharpness, on both sides, than a theory in meteorology might have been expected to call forth. For this Dr HUTTON had least apology, if greatest indulgence, on the score of temper, is due to the combatant who has the worst argument. The merits of the question cannot be considered here: It is sufficient to remark, that they came ultimately to rest on a single point, Whether the refrigeration of air is carried on by the mixture of the cold and the hot air, or by the passage of the heat itself, without such mixture, from one portion of air to another. If the former holds, Dr HUTTON's theory is established; if the latter be true, M. DE LUC's objections may at least merit examination.

Now, it is certain, that if not the only, yet almost the only, communication of heat through fluids, is produced by the mixture of one part of the fluid with another. The statical principle by which heat is thus propagated, was first, I believe, accurately explained by Dr BLACK, and since his time has been farther illustrated by the experiments of Count RUMFORD. These last have led their ingenious author to conclude that heat has no tendency to pass through fluids, otherwise than by the mixture of the parts of different temperature. The accuracy of this conclusion, in its full extent, may reasonably be questioned; but this much of it is undoubtedly true, that when the particles of a body are at liberty to move freely among themselves, the direct communication of heat, compared with the statical, is evanescent, and may be regarded as a mere infinitesimal. M. DE LUC's objections are therefore of no weight.

THE *Theory of Rain* was republished by Dr HUTTON in his *Physical Dissertations* several years afterwards, together with his answers to M. DE LUC, and several other meteorological tracts,

which contain many excellent examples of generalization, in a branch of natural history where it is more easy to accumulate facts, and more difficult to ascertain principles, than in any other \*.

AFTER

\* It may be proper to mention here some useful observations in meteorology which Dr HUTTON made, but of which he has given no account in any of his publications.

HE was, I believe, the first who thought of ascertaining the medium temperature of any climate by the temperature of the springs. With this view he made a great number of observations in different parts of Great Britain, and found, by a singular enough coincidence between two arbitrary measures, quite independent of one another, that the temperature of springs, along the east coast of this island, varies nearly at the rate of a degree of FAHRENHEIT'S thermometer for a degree of latitude. This rate of change, though it cannot be general over the whole earth, is probably not far from the truth for all the northern part of the temperate zone.

FOR estimating the effect which height above the level of the sea has in diminishing the temperature, he also made a series of observations at a very early period. By these observations he found that the difference between the state of the thermometer in two places of a given difference of level, and not very distant, in a horizontal direction, is a constant quantity, or one which remains at all seasons nearly the same, and is about  $1^{\circ}$  for 230 feet of perpendicular height.

I MUST, however, observe, that on verifying these observations, I have found the rate of the decrease of temperature a little slower than this, and very nearly a degree for 250 feet. This seems to hold for a considerable height above the earth's surface, and will be found to come pretty near the truth, to the height of five or six thousand feet. It is not however probable that the diminution of the temperature is exactly proportional to the increase of elevation; and it would seem that at heights greater than the preceding, the deviation becomes sensible; the differences of heat varying in a less ratio than the differences of elevation.

IN explaining this diminution of temperature as we ascend in the atmosphere, Dr HUTTON was much more fortunate than any other of the philosophers who have considered the same subject. It is well known that the condensation of air converts part of the latent into sensible heat, and that the rarefaction of air converts part of the sensible into latent heat. This is evident from the experiment of the air-gun, and from many others. If, therefore, we suppose a given quantity of air to be suddenly transported from the surface to any height above it, the air will expand on account of the diminution of pressure, and a part of its heat becoming latent,



AFTER the period of the two publications just mentioned, Dr HUTTON made several excursions into different parts of Scotland, with a view of comparing certain results of his theory of the earth with actual observation. His account of granite, viz. that it is a substance which, having been reduced into fusion by subterraneous heat, has been forcibly injected among the strata already consolidated, was so different from that of other mineralogists, that it seemed particularly to require farther examination. He concluded, that if this account was just, some confirmation of it must appear at those places where the granite and the strata are in contact, or where the former emerges from beneath the latter. In such situations, one might expect veins of the stone which had been in fusion to penetrate into the stone which had been solid; and some imperfect descriptions of granitic veins gave reason to imagine that this phenomenon was actually to be observed. Dr HUTTON was anxious that an *instantia crucis* might subject his theory to the severest test.

ONE

latent, it will become colder than before. Thus also, when a quantity of heat ascends by any means whatever, from one stratum of air to a superior stratum, a part of it becomes latent, so that an equilibrium of heat can never be established among the strata; but those which are less, must always remain colder than those that are more, compressed. This was Dr HUTTON's explanation, and it contains no hypothetical principle whatsoever.

To one who considers meteorology with attention, the want of an accurate hygrometer can never fail to be a subject of regret. The way of supplying this deficiency which Dr HUTTON practised was by moistening the ball of a thermometer, and observing the degree of cold produced by the evaporation of the moisture. The degree of cold, *cæteris paribus*, will be proportional to the dryness of the air, and affords, of course, a measure of that dryness. The same contrivance, but without any communication whatsoever, occurred afterwards to Mr LESLIE, and being pursued through a series of very accurate and curious experiments, has produced an instrument which promises to answer all the purposes of photometry, as well as hygrometry, and so to make a very important addition to our physical apparatus.

ONE of the places where he knew that a junction of the kind he wished to examine must be found, was the line where the great body of granite which runs from Aberdeen westward, forming the central chain of the Grampians, comes in contact with the schistus which composes the inferior ridges of the same mountains toward the south. The nearest and most accessible point of this line seemed likely to be situated not far to the eastward of Blair in Athol, and could hardly fail to be visible in the beds of some of the most northern streams which run into the Tay. Dr HUTTON having mentioned these circumstances to the Duke of ATHOL, was invited by that nobleman to accompany him in the shooting season into Glentilt, which he did accordingly, together with his friend Mr CLERK of Elden, in summer 1785.

THE Tilt is, according to the seasons, a small river, or an impetuous torrent, which runs through a glen of the same name, nearly south-west, and deeply intersects the southern ridges of the Grampian Mountains. The rock through which its bed is cut is in general a hard micaceous schistus; and the glen presents a scene of great boldness and asperity, often embellished, however, with the accompaniments of a softer landscape.

WHEN they had reached the Forest Lodge, about seven miles up the valley, Dr HUTTON already found himself in the midst of the objects which he wished to examine. In the bed of the river, many veins of red granite, (no less, indeed, than six large veins in the course of a mile), were seen traversing the black micaceous schistus, and producing, by the contrast of colour, an effect that might be striking even to an unskilful observer. The sight of objects which verified at once so many important conclusions in his system, filled him with delight; and as his feelings, on such occasions, were always strongly expressed, the guides who accompanied him were convinced that it must be nothing less

less than the discovery of a vein of silver or gold, that could call forth such strong marks of joy and exultation.

Dr HUTTON has described the appearances at this spot in the third volume of the *Edinburgh Transactions*, p. 79. and some excellent drawings of them were made by Mr CLERK, whose pencil is not less valuable in the sciences than in the arts. On the whole, it is certain, that of all the junctions of granite and schistus which are yet known, this at Glentilt speaks the most unambiguous language, and most clearly demonstrates the violence with which the granitic veins were injected among the schistus\*.

In the year following, Dr HUTTON and Mr CLERK also visited Galloway, in search of granitic veins, which they found at two different places, where the granite and schistus come in contact. One of these junctions was afterwards very carefully examined by Sir JAMES HALL and Mr DOUGLAS, now Lord SELKIRK, who made the entire circuit of a tract of granite country, which reaches from the banks of Loch Ken, where the junction is best seen, westward to the valley of Palnure, occupying a space of about 11 miles by 7. See *Edinburgh Transactions*, vol. III. *History*, p. 8.

IN summer 1787, Dr HUTTON visited the island of Arran in the mouth of the Clyde, one of those spots in which nature has collected, within a very small compass, all the phenomena most interesting to a geologist. A range of granite mountains, placed

\* I MUST take this opportunity of correcting a mistake which I have made in describing the junction in Glentilt, (*Illustrations of the Huttonian Theory*, p. 310.) where I have said, that the great body of granite from which these veins proceed, is not immediately visible. This, however, is not the fact, for the mountains on the north side of the glen are a mass of granite to which the veins can be directly traced. This I have been assured of by Mr CLERK. Dr HUTTON has not described it distinctly; and not having seen the union of the veins with the granite on the north side, when I visited the same spot, I concluded too hastily, that it had not yet been discovered.

ced in the northern part of the island, have their sides covered with primitive schistus of various kinds, to which, on the sea-shore, succeed secondary strata of grit, limestone, and even coal. Here, therefore, Dr HUTTON had another opportunity of examining the junction of the granite and schistus, and found abundance of the veins of the former penetrating into the latter. In three different places he met with this phenomenon; in the torrents that descend from the south side of Goatfield; in Glenrosa, on the west, and in the little river Sannax, on the north-east, of that mountain. From the first of these he brought a specimen of some hundred weight, consisting of a block of schistus, which includes a large vein of granite.

At the northern extremity of the island he had likewise a view of the secondary strata lying upon the primary, with their planes at right angles to one another. In the great quantity, also, of pudding-stone, containing rounded quartz gravel, united by an arenaceous cement; in the multitude of whinstone dikes, which abound in this island; and in the veins of pitchstone, a fossil which he had not before met with in its native place; he found other interesting subjects of observation; so that he returned from this tour highly gratified, and used often to say that he had no where found his expectations so much exceeded, as in the grand and instructive appearances with which nature has adorned this little island.

MR JOHN CLERK, the son of his friend MR CLERK of Elden, accompanied him in this excursion, and made several drawings, which, together with a description of the island drawn up afterwards by Dr HUTTON, still remain in manuscript.

THE least complete of the observations at Arran was that of the junction of the primitive with the secondary strata, which is but indistinctly seen in that island; and only at one place. Indeed, the contact of these two kinds of rock, though it forms a line circumscribing the bases of all primitive countries;

countries, is so covered by the soil, as to be visible in very few places. In the autumn of this same year, however, Dr HUTTON had an opportunity of observing another instance of it in the bank of the river Jedd, about a mile above the town of Jedburgh. The schistus there is micaceous, in vertical plates, running from east to west, though somewhat undulated. Over these is extended a body of red sandstone, in beds nearly horizontal, having interposed between it and the vertical strata a breccia full of fragments of these last. Dr HUTTON has given an account of this spot in the first volume of his *Theory of the Earth*, p. 432., accompanied with a copper-plate, from a drawing by Mr CLERK.

IN 1788 he made some other valuable observations of the same kind. The ridge of the Lammer-muir Hills, in the south of Scotland, consists of primary micaceous schistus, and extends from St Abb's-head westward, till it join the metalliferous mountains about the sources of the Clyde. The sea-coast affords a transverse section of this alpine tract at its eastern extremity, and exhibits the change from the primary to the secondary strata, both on the south and on the north. Dr HUTTON wished particularly to examine the latter of these, and on this occasion Sir JAMES HALL and I had the pleasure to accompany him. We sailed in a boat from Dunglass, on a day when the fineness of the weather permitted us to keep close to the foot of the rocks which line the shore in that quarter, directing our course southwards, in search of the termination of the secondary strata. We made for a high rocky point or head-land, the SICCAR, near which, from our observations on shore, we knew that the object we were in search of was likely to be discovered. On landing at this point, we found that we actually trode on the primeval rock, which forms alternately the base and the summit of the present land. It is here a micaceous schistus, in beds nearly vertical, highly indurated, and stretch-

ing from S. E. to N. W. The surface of this rock runs with a moderate ascent from the level of low-water, at which we landed, nearly to that of high-water, where the schistus has a thin covering of red horizontal sandstone laid over it; and this sandstone, at the distance of a few yards farther back, rises into a very high perpendicular cliff. Here, therefore, the immediate contact of the two rocks is not only visible, but is curiously dissected and laid open by the action of the waves. The rugged tops of the schistus are seen penetrating into the horizontal beds of sandstone, and the lowest of these last form a breccia containing fragments of schistus, some round and others angular, united by an arenaceous cement.

DR HUTTON was highly pleased with appearances that set in so clear a light the different formations of the parts which compose the exterior crust of the earth, and where all the circumstances were combined that could render the observation satisfactory and precise. On us who saw these phenomena for the first time, the impression made will not easily be forgotten. The palpable evidence presented to us, of one of the most extraordinary and important facts in the natural history of the earth, gave a reality and substance to those theoretical speculations, which, however probable, had never till now been directly authenticated by the testimony of the senses. We often said to ourselves, What clearer evidence could we have had of the different formation of these rocks, and of the long interval which separated their formation, had we actually seen them emerging from the bosom of the deep? We felt ourselves necessarily carried back to the time when the schistus on which we stood was yet at the bottom of the sea, and when the sandstone before us was only beginning to be deposited, in the shape of sand or mud, from the waters of a superincumbent ocean. An epocha still more remote presented itself, when even the most ancient of these rocks, instead of standing upright in vertical  
beds,

beds, lay in horizontal planes at the bottom of the sea, and was not yet disturbed by that immeasurable force which has burst asunder the solid pavement of the globe. Revolutions still more remote appeared in the distance of this extraordinary perspective \*. The mind seemed to grow giddy by looking so far into the abyss of time ; and while we listened with earnestness and admiration to the philosopher who was now unfolding to us the order and series of these wonderful events, we became sensible how much farther reason may sometimes go than imagination can venture to follow. As for the rest, we were truly fortunate in the course we had pursued in this excursion ; a great number of other curious and important facts presented themselves, and we returned, having collected, in one day, more ample materials for future speculation, than have sometimes resulted from years of diligent and laborious research.

IN the latter part of this same summer, (1788), Dr HUTTON accompanied the Duke of ATHOL to the Isle of Man, with a view of making a mineral survey of that island. What he saw there, however, was not much calculated to illustrate any of the great facts in geology. He found the main body of the island to consist of primitive schistus, much inclined, and more intersected with quartz veins than the corresponding schistus in the south of Scotland. In two places on the opposite sides of the island, this schistus was covered by secondary strata, but the junction was nowhere visible. Some granite veins were observed in the schistus, and many loose blocks of that stone were met with in the soil, or on the surface, but no mass of it was to be seen in its native place. The direction of the primitive strata corresponded very well with that in Galloway, running near-

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\* For a fuller deduction of the conclusions here referred to, see *Theory of the Earth*, Vol. I. p. 458. ; also *Illustrations of the Huttonian Theory*, p. 213.

ly from east to west. This is all the general information which was obtained from an excursion, which, in other respects, was very agreeable. Dr HUTTON performed it in company with his friend Mr CLERK, and they again experienced the politeness and hospitality of the same nobleman who had formerly entertained them, on an expedition which deserves so well to be remembered in the annals of geology.

THOUGH from the account now given, it appears that Dr HUTTON's mind had been long turned with great earnestness to the study of the theory of the earth, he had by no means confined his attention to that subject, but had directed it to the formation of a general system, both of physics and metaphysics \*. He tells us himself, that he was led to the study of general physics, from those views of the properties of body which had occurred to him in the prosecution of his chemical and mineralogical inquiries. In those speculations, therefore, that extended so far into the regions of abstract science, he began from chemistry; and it was from thence that he took his departure in his circum-navigation both of the material and intellectual world.

THE chemist, indeed, is flattered more than any one else with the hopes of discovering in what the essence of matter consists; and Nature, while she keeps the astronomer and the mechanician at a great distance, seems to admit him to more familiar converse, and to a more intimate acquaintance with her secrets. The vast power which he has acquired over matter, the astonishing

\* AT what time these last speculations began to share his attention with the former, I have not been able to discover, though I have reason to believe that before I became acquainted with him, which was about 1781, he had completed a manuscript treatise on each of them, the same nearly that he afterwards gave to the world. His speculations on general physics were of a date much earlier than this.

THE Physical System, referred to here, forms the third part of a work, entitled, *Dissertations on different Subjects in Natural Philosophy*, in one vol. 4to, 1792.



ing transformations which he effects, his success in analyzing almost all bodies, and in reproducing so many, seem to promise that he shall one day discover the essence of a substance which he has so thoroughly subdued; that he shall be able to bind PROTEUS in his cave, and finally extort from him the secret of his birth; in a word, that he shall find out what matter is, of what elements it is composed, and what are the properties essential to its existence.

IN entering upon this new inquiry, Dr HUTTON was forcibly struck with the very just reflection, That we do by no means explain the nature of body, when we describe it as made up of small particles; because if we allow to these particles any magnitude whatsoever, we do no more than affirm that great bodies are made up of small ones. The elements of body must, therefore, be admitted to be something unextended. To these unextended elements, Dr HUTTON gave the name of MATTER, and carefully distinguished between that term and the term BODY, which he applied only to those combinations of matter that are necessarily conceived to possess impenetrability, extension and inertia.

THE most accurate examination of the properties of body confirms the truth of the opinion, that it is composed of unextended elements. Bodies may be compressed into smaller dimensions; many by the application of mechanical force, and all by the diminution of their heat: nor is there any limit to this compression, or any point beyond which the farther reduction of volume becomes impossible. This holds of substances the most compact; as well as the most volatile and elastic, and clearly evinces that the elements of body are not in contact with one another, and that in reality we perceive nothing in body but the existence of certain powers or forces, acting with various intensities, and in various directions. Thus the supposed impenetrability, and of course the extension of body, is nothing else than the effort of a resisting

ing or repulsive power ; its cohesion, weight, &c. the efforts of attractive power ; and so with respect to all its other properties.

BUT if this be granted, and if it be true that in the material world every phenomenon can be explained by the existence of Power, the supposition of extended particles as a *substratum* or residence for such power, is a mere hypothesis, without any countenance from the matter of fact. For if these solid particles are never in contact with one another, what part can they have in the production of natural appearances, or in what sense can they be called the residence of a force which never acts at the point where they are present ? Such particles, therefore, ought to be entirely discarded from any theory that proposes to explain the phenomena of the material world.

THUS, it appears, that power is the essence of matter, and that none of our perceptions warrant us in considering even body as involving any thing more than force, subjected to various laws and modifications.

MATTER, taken in this sense, is to be considered as indefinitely extended, and without inertia. Its presence through all space is proved by the universality of gravitation ; and its want of inertia, by the want of resistance to the planetary motions. Thus, in our inquiry concerning physical causes, we are relieved from one great difficulty, that of supposing matter to act where it is not. The force of gravitation, according to this system, is not the action of two distant bodies upon one another, but it is the action of certain powers, diffused through all space, which may be transmitted to any distance. There seems to me, however, to remain a difficulty hardly less than that from which we appear to be relieved, *viz.* to assign a reason why the intensity with which such powers act on any body, should depend on the position and magnitude of all the bodies in the universe, and should bear to these continually the same relation. But, however this be, the ingenuity of Dr HUTTON's reasonings cannot be questioned,

ed, nor, I think, the justness of many of his conclusions. His explanations of cohesion, heat, fluidity, deserve particular attention. In one thing, however, he seems to have fallen into an error, which runs through much of his reasoning, concerning the principles of gravitation and inertia. He affirms, that "without gravity, a body endowed with all the other material qualities would have no inertia; that it would not diminish the velocity of the moving body by which it should be actuated, nor would it move a heavy body whatever were its velocity \*." Now, this proposition, though from its nature it cannot be brought to the immediate test of experience, is certainly inconsistent with the principles of mechanics; at the same time, it is true, that we would not, in the case here supposed, have the same means of measuring the motion lost, or gained by collision, which we have in the actual state of bodies. This is perhaps what misled Dr HUTTON; and though his remarks on the measures of motion and force are very acute, and many of them very just, the mathematical reader will regret the want of that mode of reasoning, which has raised mechanics to so high a rank among the sciences.

It is impossible not to remark the affinity of this theory with that of the celebrated BOSCOVICH, in which, as in this, all the phenomena of the material world are explained, by the supposition of forces variously modified, and without the assistance of solid or extended particles. These forces are supposed to be arranged round mathematical points, which are moveable, and act on one another by means of the forces surrounding them. A most ingenious application of this principle is made to all the usual researches of the mechanical philosophy, and; it must be confessed, that few theories have more beauty and simplicity to recommend them, or do better assist the imagination in the explanation.

\* *Dissertations*, &c. p. 312. § 31.

planation of natural appearances. But it involves, in the whole of it, this great difficulty, that mathematical points are not only capable of motion, but capable of being endowed, or, at least, distinguished, by physical qualities. Dr HUTTON, in his theory, has avoided this difficulty, by giving no other than a negative definition of the MATTER which he supposes the elementary principle of body. On this account, though to the imagination his theory may want the charms which the other possesses, yet it has the advantage of going just to the extent to which our perceptions or our observations authorise us to proceed, and of being accurately circumscribed by the limits pointed out by the laws of philosophical induction\*.

THE existence of matter neither heavy nor inert, which he had taken so much pains to establish, was applied by him to explain

\* THOUGH BOSCOVICH'S Theory was published long before Dr HUTTON'S, so early, indeed, as the year 1758, there is no reason to think that the latter was in any degree suggested by the former. BOSCOVICH'S theory was hardly known in this country till about the year 1770, and the first sketches of Dr HUTTON'S theory are of a much older date. Besides, the method of reasoning pursued by the authors is quite different; and their conclusions, though alike in some things, directly contrary in others, as in what regards gravity, inertia, &c. The MONADS of LEIBNITZ might more reasonably be supposed to have pointed out to Dr HUTTON the necessity of supposing the elements of body to be unextended, if the originality of his own conceptions, and the little regard he paid to authority in matters of theory, did not relieve us from the necessity of looking to others for the sources of his opinions.

THE principal defect of his theory seems to me to consist in this, that it does not state with precision the difference between the constitution of those *powers* which simply form matter, and those that form the more complex substance, body. In other words, it does not explain what must be added to matter to make it body. The answer seems to me to be, that the addition of a repelling power, in all directions, is sufficient for that purpose. Such a repulsion, if strong enough, would produce both impenetrability and inertia. The matter, again, that possessed only an attractive power, like *gravity*, or a repulsive power only in a certain direction, like *light*, would not be inert nor impenetrable. In this inference, however, from his system, I am not sure if I should meet with the author's approbation.

plain the phenomena of light, heat, and electricity. He considered all these three as modifications of the solar substance, and thought that many of the appearances they exhibit, are only to be explained on the supposition that they consist of an expansive force, of which inertness is not predicable; in particular, that light is a power propagated from the sun in all directions, like gravity, with this difference, that it is repulsive, while gravity is attractive, and requires time for its transmission, which the latter does not, at least in any sensible quantity\*.

THE prosecution of this subject has led him to consider the nature of PHLOGISTON, a substance once so famous in chemistry, but of which the name has almost as entirely disappeared from the vocabulary of that science, as the word Vortex from the language of physical astronomy. The new and important experiments made on the calcination of metals, and on the composition of water, are, as is well known, the foundations of the antiphlogistic theory. Nobody was more pleased than Dr HUTTON with these experiments, nor held in higher estimation the character and abilities of the chemists and philosophers by whom they were conducted. He was nevertheless of opinion, that the conclusions drawn from them are not altogether unexceptionable, nor deduced with a sufficient attention to every circumstance. This remark he thought peculiarly applicable to what regards the composition of water, to the phenomena of which experiment, the dissertation we are now speaking of is chiefly directed. The two æriform fluids, it is there observed, which compose water, in order to unite, must not simply be brought together, for in that state they might remain for ever unchanged, but they must be set on fire, and made to burn, and from this burning there are evidently two substances which make their escape, VOL. V.—P. III. L namely,

\* See Dissertations V. and VI. on Matter and Motion, in the work above quoted. The Chemical Dissertation on Phlogiston is in the same volume, p. 171.

namely, Light and Heat. Though, therefore, the weight of the water generated, and of the gases combined, may be admitted to be equal, yet it must be acknowledged, that two substances are lost, which the chemist cannot confine in his closest vessel, nor weigh in his finest balance, and it is going much farther than we are authorised to do, either by experiment or analogy, to conclude that these substances have had no effect. As heat and light, in Dr HUTTON's system, are composed of that matter which does not gravitate, the exact coincidence which M. LAVOISIER observed between the weight of the water produced and that of the two elastic fluids, united in the composition of it, was no argument, in his eyes, against the escape of a very essential part of the ingredients.

PURSUING the same reasoning, he shews how little ground there is to suppose that the heat and light evolved in this experiment proceed from the vital air; and he concludes, that the real explanation of the process is, that by burning, the matter of light and heat, or the phlogiston of the hydrogenous gas, is set at liberty, and is thus enabled to unite with the vital air.

IN the same manner, on examining what relates to the burning of inflammable bodies, he finds the oxygenous gas unequal to the effect of furnishing by its latent heat, or caloric, the whole of the sensible heat that is produced. He concludes, therefore, that the hypothesis of the existence of phlogiston in those bodies that are termed inflammable, is necessary to account for the phenomena of burning; phenomena, as he justly remarks, which are among the most curious and important of any that are exhibited by the material world. On the whole, it cannot be doubted, that great ingenuity and much sound argument are displayed throughout the whole of this dissertation, and that whatever be ultimately decided with regard to the principle for which the author so strenuously contends, he has made it evident, that the  
conclusions

conclusions of the antiphlogistic theory have been drawn with too much precipitancy, and carried farther than is warranted by the strict rules of inductive philosophy.

THE subject of Fire, Light and Heat was resumed by Dr HUTTON several years after this period, and formed the subject of a series of papers which he read in the Royal Society of Edinburgh, and afterwards published separately. He there explains more fully his notion of the substances just mentioned, which he considers as different modifications of the solar matter, alike destitute of inertness and of gravity.

A MORE voluminous work from Dr HUTTON's pen, made its appearance soon after the Physical Dissertations, viz. *An Investigation of the Principles of Knowledge, and of the Progress of Reason from Sense to Science and Philosophy*, in three volumes quarto.

HE informs us himself of the train of thought by which he was led to the metaphysical speculations contained in these volumes. He had satisfied himself, by his physical investigations, that body is not what it is conceived by us to be, a thing necessarily possessing volume, figure and impenetrability, but merely an assemblage of powers, that by their action produce in us the ideas of these external qualities. His curiosity, therefore, was naturally excited to inquire farther into the manner in which we form our conceptions of body, or into the nature of the intercourse which the mind holds with those things that exist without it. In pursuing this inquiry, he soon became convinced, that magnitude, figure and impenetrability, are no otherwise perceived by the mind than colour, taste and smell; that is, that what are called the primary qualities of body, are precisely on the same footing with the secondary, and are both conceptions of the mind, which can have no resemblance to the external cause by which those conceptions are produced. The world, therefore, as conceived by us, is the creation of the mind itself, but of the mind acted on from without, and receiving informa-

tion from some external power. But though, according to this reasoning, there be no resemblance between the world without us, and the notions that we form of it, though magnitude and figure, though space, time and motion, have no existence but in the mind; yet our perceptions being consistent, and regulated by constant and uniform laws, are as much realities to us, as if they were the exact copies of things really existing; they equally interest our happiness, and must equally determine our conduct. They form a system, not dependent on the mind alone, but dependent on the action which certain external causes have upon it. The whole doctrine, therefore, of moral obligation, remains the same in this system, and in that which maintains the perfect resemblance of our ideas to the causes by which they are produced.

MANY philosophers have regarded our ideas as very imperfect representations of external things; but Dr HUTTON considers their perfect dissimilitude as completely proved. PLATO has likened the mind to an eye, so situated, as to see nothing but the faint images of objects projected on the bottom of a dark cave, while the objects themselves are entirely concealed; but he thinks, that by help of philosophy, the mental eye may be directed toward the mouth of the cave, and may perceive the objects in their true figure and dimensions. But, with Dr HUTTON, the figures seen at the bottom of the cave have no resemblance to the originals without; nor can man, by any contrivance, hold communication with those originals, nor ever know any thing about them, except that they are not what they seem to be, and have no property in common with the figures which denote their existence. In a word, external things are no more like the perceptions they give rise to, than wine is similar to intoxication, or opium to the delirium which it produces.

IT has been already remarked, that this system, however peculiar in other respects, involves in it the same principles of morals



ral's with those more generally received ; and the same may be said as to the existence of GOD, and the immortality of the soul. The view which it presents of the latter doctrine, deserves particularly to be remarked. Death is not regarded here as the dissolution of a connexion between mind, and that system of material organs, by means of which it communicated with the external world, but merely as an effect of the mind's ceasing to perceive a particular order or class of things ; it is therefore only the termination of a certain mode of thought ; and the extinction, not of any mental power, but of a train of conceptions, which, in consequence of external impulse, had existed in the mind. Thus, as nothing essential to intellectual power perishes, we are to consider death only as a passage from one condition of thought to another ; and hence this system appeared, to the author of it, to afford a stronger argument than any other, for the existence of the mind after death.

INDEED, Dr HUTTON has taken great pains to deduce from his system, in a regular manner, the leading doctrines of morality and natural religion, having dedicated the third volume of his book almost wholly to that object. It is worthy of remark, that while he is thus employed, his style assumes a better tone, and a much greater degree of perspicuity, than it usually possesses. Many instances might be pointed out, where the warmth of his benevolent and moral feelings bursts through the clouds that so often veil from us the clearest ideas of his understanding. One, in particular, deserves notice, in which he treats of the importance of the female character to society, in a state of high civilization \*. A felicity of expression, and a flow of natural eloquence, inspired by so interesting a subject, make us regret that his pen did not more frequently do justice to his thoughts.

THE metaphysical theory, of which the outline, (though very imperfectly), has now been traced, cannot fail to recall the opinions

\* *Investigation of the Principles of Knowledge*, Vol. III. p. 588. &c.

nions maintained by Dr BERKELEY concerning the existence of matter. The two systems do indeed agree in one material point, but differ essentially in the rest. They agree in maintaining, that the conceptions of the mind are not copied from things of the same kind existing without it; but they differ in this, that Dr BERKELEY imagined that there is nothing at all external, and that it is by the direct agency of the Deity that sensation and perception are produced in the mind. Dr HUTTON holds, on the other hand, that there is an external existence, from which the mind receives its information, and by the action of which, impressions are made on it; but impressions that do not at all resemble the powers by which they are caused.

THE reasonings also by which the two theories are supported, are very dissimilar, though perhaps they so far agree, that if Dr BERKELEY had been better acquainted with physics, and had made it more a rule to exclude all hypothesis, he would have arrived precisely at the same conclusion with Dr HUTTON. Indeed, I cannot help being of opinion, that every one will do so, who, in investigating the origin of our perceptions, determines to reason without assuming any hypothesis, and without taking for granted any of those maxims which the mind is disposed to receive, either, as some philosophers say, from habit, or, as others maintain, from an instinctive determination, (such as has been termed *common sense*) that admits of no analysis. Though this may not be the kind of reasoning best suited to the subject, yet it is so analogous to what succeeds in other cases, that it is good to have an example of it, and, on that account, were it for nothing else, the theory we are now speaking of certainly merits more attention than it has yet met with \*. The great size of the book, and the  
obscurity

\* I HAVE hardly found this work of Dr HUTTON's quoted by any writer of eminence, except by Dr PAR, in his *Spital Sermon*, a tract no less remarkable for learning and acuteness, than for the liberality and candour of the sentiments which it contains.

obscurity which may justly be objected to many parts of it, have probably prevented it from being received as it deserves, even among those who are conversant with abstract speculation. An abridgment of it, judiciously executed, so as to state the argument in a manner both perspicuous and concise, would, I am persuaded, make a valuable addition to metaphysical science.

THE publication of this work was Dr HUTTON's occupation on his recovery from a severe illness, with which he was seized in summer 1793. Before this time he had enjoyed a long continuance of good health, and great activity both of body and mind. The disorder that now attacked him, (a retention of urine), was one of those that most immediately threaten life, and he was preserved only by submitting to a dangerous, and painful operation. He was thus reduced to a state of great weakness, and was confined to his room for many months. By degrees, however, the goodness of his constitution, aided, no doubt, by the vigour and elasticity of his mind, restored him to a considerable measure of health, and rendered his recovery much more complete than could have been expected. One of his amusements, when he had regained some tolerable degree of strength, was in superintending the publication, and correcting the proof-sheets of the work just mentioned.

DURING his convalescence, his activity was farther called into exertion, by an attack on his *Theory of the Earth*, made by Mr KIRWAN, in the *Memoirs of the Irish Academy* \*, and rendered formidable,

\* THIS was not the first attack which had been made on his theory, for M. DE LUC, in a series of letters, inserted in the *Monthly Review* for 1790 and 1791, had combated several of the leading opinions contained in it. To these Dr HUTTON made no other reply, than is to be met with occasionally in the enlarged edition of his *Theory*, published four years afterwards. If I do not mistake, however, he intended a more particular answer, and actually sent one to the editors of

dable, not by the strength of the arguments it employed, but by the name of the author, the heavy charges which it brought forward, and the gross misconceptions in which it abounded\*.

BEFORE this period, though Dr HUTTON had been often urged by his friends to publish his entire work on the *Theory of the Earth*, he had continually put off the publication, and there seemed to be some danger that it would not take place in his own life time. The very day, however, after Mr KIRWAN's paper was put into his hands, he began the revival of his manuscript, and resolved immediately to send it to the press. The reason he gave was, that Mr KIRWAN had in so many instances completely mistaken, both the facts, and the reasonings in his *Theory*, that he saw the necessity of laying before the world a more ample explanation of them. The work was accordingly published, in two volumes octavo, in 1795; and contained, besides what was formerly given in the *Edinburgh Transactions*, the proofs and reasonings much more in detail, and a much fuller application of the principles to the explanation of appearances. The two volumes, however, then published, do not complete the theory: a third, necessary for that purpose, remained behind, and is still in manuscript.

#### AFTER

of the same *Review*, who refused to insert it. This, indeed, I do not state with perfect confidence, as I speak only from recollection, and would not, on that authority, bring a positive charge of partiality against men who exercise a profession in which impartiality is the first requisite. Supposing, however, the statement here given to be correct, an excuse is still left for the Reviewers; they may say, that in communicating original papers, as they do not act in their judicial capacity, they are not bound to dispense justice with their usual blindness and severity, but may be permitted to relax a little from the exercise of a virtue that is so often left to be its own reward.

\* FOR a defence of Dr HUTTON against the charges here alluded to, I must take the liberty of referring to the *Illustrations of the Huttonian Theory*, p. 119. and 125,

AFTER the publication of the work just mentioned, he began to prepare another for the press, on a subject which had early occupied his thoughts, and had been at no time of his life entirely neglected. This subject was husbandry, on which he had written a great deal, the fruit both of his reading and experience; and he now proposed to reduce the whole into a systematic form, under the title of *Elements of Agriculture*. This work, which he nearly completed, remains in manuscript. It is written with considerable perspicuity; and though I can judge but very imperfectly of its merits, I can venture to say, that it contains a great deal of solid and practical knowledge, without any of the vague and unphilosophic theory so common in books on the same subject. In particular, I must observe, that where it treats of climate, and the influence of heat, in accelerating the maturity of plants, it furnishes several views that appear to be perfectly new, and that are certainly highly interesting.

THE period, however, was now not far distant, which was to terminate the exertions of a mind of such singular activity, and of such ardour in the pursuit of knowledge. Not long after the time we are speaking of, Dr HUTTON was again attacked by the same disorder from which he had already made so remarkable a recovery. He was again saved from the danger that immediately threatened him, but his constitution had materially suffered, and nothing could restore him to his former strength. He recovered, indeed, so far as to amuse himself with study, and with the conversation of his friends, and even to go on with the work on agriculture, which was nearly completed. He was, however, confined entirely to the house; and in the course of the winter 1796-7, he became gradually weaker, was extremely emaciated, and suffered much pain, but still retained the full activity and acuteness of his mind. He constantly employed himself in reading and writing, and was particularly pleased with the third and fourth volumes of SAUSSURE'S *Voyages*

*aux Alpes*, which reached him in the course of that winter, and became the last study of one eminent geologist, as they were the last work of another. On Saturday the 26th of March he suffered a good deal of pain; but, nevertheless, employed himself in writing, and particularly in noting down his remarks on some attempts which were then making towards a new mineralogical nomenclature. In the evening he was seized with a shivering, and his uneasiness continuing to increase, he sent for his friend Mr RUSSEL, who attended him as his surgeon. Before he could possibly arrive, all medical assistance was in vain: Dr HUTTON had just strength left to stretch out his hand to him, and immediately expired.

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Dr HUTTON possessed, in an eminent degree, the talents, the acquirements, and the temper, which entitle a man to the name of a philosopher. The direction of his studies, though in some respects irregular and uncommon, had been highly favourable to the developement of his natural powers, especially of that quick penetration, and that originality of thought, which strongly marked his intellectual character. From his first outset in science, he had pursued the track of experiment and observation, and it was not till after being long exercised in this school, that he entered on the field of general and abstract speculation. He combined accordingly, through his whole life, the powers of an accurate observer, and of a sagacious theorist, and was as cautious and patient in the former character, as he was bold and rapid in the latter.

LONG and continued practice had increased his powers of observation to a high degree of perfection; so that, in discriminating mineral substances, and in seizing the affinities or differences

ferences among geological appearances, he had an acuteness hardly to be excelled. The eulogy so happily conveyed in the Italian phrase, of *osservatore oculatissimo*, might most justly be applied to him; for, with an accurate eye for perceiving the characters of natural objects, he had in equal perfection the power of interpreting their signification, and of decyphering those ancient hieroglyphics which record the revolutions of the globe. There may have been other mineralogists, who could describe as well the fracture, the figure, the smell, or the colour of a specimen; but there have been few who equalled him in reading the characters, which tell not only what a fossil *is*, but what it *has been*, and declare the series of changes through which it has passed. His expertness in this art, the fineness of his observations, and the ingenuity of his reasonings, were truly admirable. It would, I am persuaded, be difficult to find in any of the sciences a better illustration of the profound maxims established by BACON, in his *Prærogativæ Instantiarum*, than were often afforded by Dr HUTTON's mineralogical disquisitions, when he exhibited his specimens, and discoursed on them with his friends. No one could better apply the luminous instances to elucidate the obscure, the decisive to interpret the doubtful, or the simple to unravel the complex. None was more skilful in marking the gradations of nature, as she passes from one extreme to another; more diligent in observing the *continuity* of her proceedings, or more sagacious in tracing her footsteps, even where they were most lightly impressed.

WITH him, therefore, mineralogy was not a mere study of names and external characters, (though he was singularly well versed in that study also), but it was a sublime and important branch of physical science, which had for its object to unfold the connexion between the past, the present, and the future conditions of the globe. Accordingly, his *collection of fossils* was formed for explaining the principles of geology, and for illustrating

ting the changes which mineral substances have gone through, in the passage which, according to all theories, they have made, from a soft or fluid, to a hard and solid state, and from immersion under the ocean, to elevation above its surface. The series of these changes, and the relative antiquity of the different steps by which they have been effected, were the objects which he had in view to explain; and his cabinet, though well adapted to this end, with regard to other purposes was very imperfect. They who expect to find, in a collection, specimens of all the species, and all the varieties, into which a system of artificial arrangement may have divided the fossil kingdom, will perhaps turn fastidiously from one that is not remarkable either for the number or brilliancy of the objects contained in it. They, on the other hand, will think it highly interesting, who wish to reason concerning the natural history of minerals, and who are not less eager to become acquainted with the laws that govern, than with the individuals that compose, the fossil kingdom.

THE loss sustained by the death of Dr HUTTON, was aggravated, to those who knew him, by the consideration of how much of his knowledge had perished with himself, and, notwithstanding all that he had written, how much of the light collected by a long life of experience and observation, was now completely extinguished. It is indeed melancholy to reflect, that with all who make proficiency in the sciences founded on nice and delicate observation, something of this sort must unavoidably happen. The experienced eye, the power of perceiving the minute differences, and fine analogies, which discriminate or unite the objects of science; and the readiness of comparing new phenomena with others already treasured up in the mind; these are accomplishments which no rules can teach, and no precepts can put us in possession of. This is a portion of knowledge which every man must acquire for himself, and which nobody can leave as an inheritance to his successor. It seems, indeed, as if nature had



had in this instance admitted an exception to the rule, by which she has ordained the perpetual accumulation of knowledge among civilized men, and had destined a considerable portion of science continually to grow up and perish with the individual.

A CIRCUMSTANCE which greatly distinguished the intellectual character of the philosopher of whom we now speak, was an uncommon activity and ardour of mind, upheld by the greatest admiration of whatever in science was new, beautiful, or sublime. The acquisitions of fortune, and the enjoyments which most directly address the senses, do not call up more lively expressions of joy in other men, than hearing of a new invention, or being made acquainted with a new truth, would, at any time, do in Dr HUTTON. This sensibility to intellectual pleasure, was not confined to a few objects, nor to the sciences which he particularly cultivated : he would rejoice over WATT's improvements on the steam-engine, or Cook's discoveries in the South Sea, with all the warmth of a man who was to share in the honour or the profit about to accrue from them. The fire of his expression, on such occasions, and the animation of his countenance and manner, are not to be described ; they were always seen with great delight by those who could enter into his sentiments, and often with great astonishment by those who could not.

WITH this exquisite relish for whatever is beautiful and sublime in science, we may easily conceive what pleasure he derived from his own geological speculations. The novelty and grandeur of the objects offered by them to the imagination, the simple and uniform order given to the whole natural history of the earth, and, above all, the views opened of the wisdom that governs nature, are things to which hardly any man could be insensible ; but to him they were matter, not of transient delight,  
but

but of solid and permanent happiness. Few systems, indeed, were better calculated than his, to entertain their author with such noble and magnificent prospects; and no author was ever more disposed to consider the enjoyment of them, as the full and adequate reward of his labours.

THE great range which he had taken in science, has sufficiently appeared, from the account already given of his works \*. There were indeed hardly any sciences, except the mathematical, to which he had not turned his attention, and his neglect of these probably arose from this, that, at the time when his acquaintance with them should have commenced, his love of knowledge had already fixed itself on other objects. The aptitude of his mind for geometrical reasoning, was, however, proved on many occasions. His theory of rain rests on mathematical principles, and the conclusions deduced from them are perfectly accurate, though by no means obvious. I may add, that he had an uncommon facility in comprehending the nature of mechanical contrivances; and, for one who was not a practical engineer, could form, beforehand, a very sound judgment concerning their effects.

NOTWITHSTANDING a taste for such various information, and a mind of such constant activity, he read but few speculative books, directing his attention chiefly to such as furnished the materials of speculation. Of voyages, travels, and books relating to the

\* He had studied with great care several subjects of which no mention is made above. One of these was the Formation, or, as we may rather call it, the Natural History of Language. A portion of his metaphysical work is dedicated to the Theory of Language, vol. I. p. 574, &c.; and vol. II. p. 624, &c. He read several very ingenious papers on the Written Language, in the Royal Society of Edinburgh, see *Transactions of the Royal Society of Edinburgh*, vol. II. Hist. p. 5. &c. The Chinese language, as an extreme case in the invention of writing, had greatly occupied his thoughts, and is the subject of several of his manuscripts.

the natural history of the earth, he had an extensive knowledge : he had studied them with that critical discussion which such books require above all others ; carefully collecting from them the facts that appeared accurate, and correcting the narratives that were imperfect, either by a comparison with one another, or by applying to them the standard of probability which his own observation and judgment had furnished him with. On the other hand, he bestowed but little attention on books of opinion and theory ; and while he trusted to the efforts of his mind for digesting the facts he had obtained from reading or experience, into a system of his own, he was not very anxious, at least till that was accomplished, to be informed of the views which other philosophers had taken of the same subject. He was but little disposed to concede any thing to mere authority ; and to his indifference about the opinions of former theorists, it is probable that his own speculations owed some part, both of their excellencies, and their defects.

As he was indefatigable in study, and was in the habit of using his pen continually as an instrument of thought, he wrote a great deal, and has left behind him an incredible quantity of manuscript, though imperfect, and never intended for the press. Indeed his manner of life, at least after he left off the occupations of husbandry, gave him such a command of his time, as is enjoyed by very few. Though he used to rise late, he began immediately to study, and generally continued busy till dinner. He dined early, almost always at home, and passed very little time at table ; for he ate sparingly, and drank no wine. After dinner he resumed his studies, or, if the weather was fine, walked for two or three hours, when he could not be said to give up study, though he might, perhaps, change the object of it. The evening he always spent in the society of his friends. No professional, and rarely any domestic arrangements interrupted this uniform course of life, so that his time was wholly divided between the pursuits  
of

of science and the conversation of his friends, unless when he travelled from home on some excursion, from which he never failed to return furnished with new materials for geological investigation.

To his friends his conversation was inestimable; as great talents, the most perfect candour, and the utmost simplicity of character and manners, all united to stamp a value upon it. He had, indeed, that genuine simplicity, originating in the absence of all selfishness and vanity, by which a man loses sight of himself altogether, and neither conceals what is, nor affects what is not. This simplicity pervaded his whole conduct; while his manner, which was peculiar, but highly pleasing, displayed a degree of vivacity, hardly ever to be found among men of profound and abstract speculation. His great liveliness, added to this aptness to lose sight of himself, would sometimes lead him into little eccentricities, that formed an amusing contrast with the graver habits of a philosophic life.

THOUGH extreme simplicity of manner does not unfrequently impart a degree of feebleness to the expression of thought, the contrary was true of Dr HUTTON. His conversation was extremely animated and forcible, and, whether serious or gay, full of ingenious and original observation. Great information, and an excellent memory, supplied an inexhaustible fund of illustration, always happily introduced, and in which, when the subject admitted of it, the witty and the ludicrous never failed to occupy a considerable place.—But it is impossible by words to convey any idea of the effect of his conversation, and of the impression made by so much philosophy, gaiety and humour, accompanied by a manner at once so animated and so simple. Things are made known only by comparison, and that which is *unique* admits of no description.

THE whole exterior of Dr HUTTON was calculated to heighten the effect which his conversation produced. His figure was  
slender

slender, but indicated activity; while a thin countenance, a high forehead, and a nose somewhat aquiline, bespoke extraordinary acuteness and vigour of mind. His eye was penetrating and keen, but full of gentleness and benignity; and even his dress, plain, and all of one colour, was in perfect harmony with the rest of the picture, and seemed to give a fuller *relief* to its characteristic features\*.

THE friendship that subsisted between him and Dr BLACK has been already mentioned, and was indeed a distinguishing circumstance in the life and character of both. There was in these two excellent men that similarity of disposition which must be the foundation of all friendship, and, at the same time, that degree of diversity, which seems necessary to give to friends the highest relish for the society of one another.

THEY both cultivated nearly the same branches of physics, and entertained concerning them nearly the same opinions. They were both formed with a taste for what is beautiful and great in science; with minds inventive, and fertile in new combinations. Both possessed manners of the most genuine simplicity, and in every action discovered the sincerity and candour of their dispositions; yet they were in many things extremely dissimilar. Ardour, and even enthusiasm, in the pursuit of science, great rapidity of thought, and much animation, distinguished Dr HUTTON on all occasions. Great caution in his reasonings, and a coolness of head that even approached to indifference, were characteristic of Dr BLACK. On attending to their conversation, and the way in which they treated any question of science or philosophy, one would say that Dr BLACK dreaded nothing so much as error, and that Dr HUTTON dread-

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\* A PORTRAIT of Dr HUTTON, by RAEBURN, painted for the late JOHN DAVIDSON, Esq; of Stewartfield, one of his old and intimate friends, conveys a good idea of a physiognomy and character of face to which it was difficult to do complete justice.

ed nothing so much as ignorance; that the one was always afraid of going beyond the truth, and the other of not reaching it. The curiosity of the latter was by much the most easily awakened, and its impulse most powerful and imperious. With the former, it was a desire which he could suspend and lay asleep for a time; with the other, it was an appetite that might be satisfied for a moment, but was sure to be quickly renewed. Even the simplicity of manner which was possessed by both these philosophers, was by no means precisely the same. That of Dr BLACK was correct, respecting at all times the prejudices and fashions of the world; that of Dr HUTTON was more careless, and was often found in direct collision with both.

FROM these diversities, their society was infinitely pleasing, both to themselves and those about them. Each had something to give which the other was in want of. Dr BLACK derived great amusement from the vivacity of his friend, the sallies of his wit, the glow and original turn of his expression; and that calmness and serenity of mind which, even in a man of genius, may border on languor or monotony, received a pleasing impulse by sympathy with more powerful emotions.

ON the other hand, the coolness of Dr BLACK, the judiciousness and solidity of his reflections, served to temper the zeal, and restrain the impetuosity of Dr HUTTON. In every material point of philosophy they perfectly agreed. The theory of the earth had been a subject of discussion with them for many years, and Dr BLACK subscribed entirely to the system of his friend. In science, nothing certainly is due to authority, except a careful examination of the opinions which it supports. It is not meant to claim any more than this in favour of the Huttonian Geology; but they who reject that system, without examination, would do well to consider, that it had the entire and unqualified approbation of one of the coolest, and  
foundest

foundest reasoners of which the present age furnishes any example.

MR CLERK of Elden was another friend, with whom, in the formation of his theory, Dr HUTTON maintained a constant communication. Mr CLERK, perhaps from the extensive property which his family had in the coal-mines near Edinburgh, was early interested in the pursuits of mineralogy. His inquiries, however, were never confined to the objects which mere situation might point out, and, through his whole life, have been much more directed by the irresistible impulse of genius, than by the action of external circumstances. Though not bred to the sea, he is well known to have studied the principles of naval war with unexampled success; and though not exercising the profession of arms, he has viewed every country through which he has passed with the eye of a soldier as well as a geologist. The interest he took in studying the surface no less than the interior of the earth; his extensive information in most branches of natural history; a mind of great resource, and great readiness of invention; made him, to Dr HUTTON, an invaluable friend and co-adjutor. It cannot be doubted, that, in many parts, the system of the latter has had great obligations to the ingenuity of the former, though the unreserved intercourse of friendship, and the adjustments produced by mutual suggestion, might render those parts undistinguishable even by the authors themselves. Mr CLERK's pencil was ever at the command of his friend, and has certainly rendered him most essential service.

BUT it was not to philosophers and men of science only that Dr HUTTON's conversation was agreeable. He was little known, indeed, in general company, and had no great relish for the enjoyment which it affords; yet he was fond of domestic society, and took great delight in a few private circles, where several excellent and accomplished individuals of both sexes thought themselves happy to be reckoned in the num-

ber of his friends. In one or other of these, he was accustomed almost every evening to seek relaxation from the studies of the day, and found always the most cordial welcome. A brighter tint of gaiety and cheerfulness spread itself over every countenance when the Doctor entered the room; and the philosopher who had just descended from the sublimest speculations of metaphysics, or risen from the deepest researches of geology, seated himself at the tea-table, as much disengaged from thought, as cheerful and gay, as the youngest of the company. These parties were delightful, and, by all who have had the happiness to be present at them, will never cease to be remembered with pleasure.

HE used also regularly to unbend himself with a few friends, in the little society alluded to in Professor STEWART's *Life of Mr SMITH*, and usually known by the name of the *Oyster Club*. This club met weekly; the original members of it were Mr SMITH, Dr BLACK, and Dr HUTTON, and round them was soon formed a knot of those who knew how to value the familiar and social converse of these illustrious men. As all the three possessed great talents, enlarged views, and extensive information, without any of the stateliness and formality which men of letters think it sometimes necessary to affect; as they were all three easily amused; were equally prepared to speak and to listen; and as the sincerity of their friendship had never been darkened by the least shade of envy; it would be hard to find an example, where every thing favourable to good society was more perfectly united, and every thing adverse more entirely excluded. The conversation was always free, often scientific, but never didactic or disputatious; and as this club was much the resort of the strangers who visited Edinburgh, from any object connected with art or with science, it derived from thence an extraordinary degree of variety and interest. It



is matter of real regret, that it has been unable to survive its founders.

THE simplicity of manner that has been already remarked as so strikingly exemplified in Dr HUTTON, was but a part of an extreme disinterestedness which manifested itself in every thing he did. He was upright, candid, and sincere; strongly attached to his friends; ready to sacrifice any thing to assist them; humane and charitable. He set no great value on money, or, perhaps, to speak properly, he set on it no more than its true value; yet, owing to the moderation of his manner of life, and the ability with which his friend Mr DAVIE conducted their joint concerns, he acquired considerable wealth.

HE was never married, but lived with his sisters, three excellent women, who managed his domestic affairs; and of whom, only one, Miss ISABELLA HUTTON, remained to lament his death. By her his collection of fossils, about which he left no particular instructions, was presented to Dr BLACK; who thought that he could not better consult the advantage of the public, or the credit of his friend, than by giving it to the Royal Society of Edinburgh, under the condition that it should be completely arranged, and kept for ever separate, for the purpose of illustrating the HUTTONIAN THEORY OF THE EARTH.

The first of these is the fact that the  
the second is the fact that the  
the third is the fact that the

the fourth is the fact that the  
the fifth is the fact that the  
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the tenth is the fact that the  
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II. MINUTES of the LIFE and CHARACTER of JOSEPH BLACK,  
M. D. Addressed to the Royal Society of Edinburgh.

[Read Aug. 3. 1801.]

THE merits of studious men are to be estimated by the aids they have given to the advancement of science, or the literary monuments they have left with posterity; but if the public be gratified by their labours in these respects, readers are generally willing also to be told, who and whence they were.

JOSEPH BLACK, the person to whom these minutes relate, successively Professor in the Universities of Glasgow and of Edinburgh, Member of this Society, and of other royal and public institutions in Europe; having made important discoveries, and having laid the foundations of many others, towards erecting a fabric of science, which has since been raised to a considerable height; and having been himself distinguished for modesty and felicity of manners, as well as correctness of understanding, and ingenuity of research,—will, it is hoped, be thought worthy of notice on these accounts. He was born on the banks of the Garrone, in France, in the year 1728. His parents were Irish and Scots. His father, JOHN BLACK, a native of Belfast in Ireland, was settled in the wine-trade at Bourdeaux. His mother was a daughter of ROBERT GORDON, of the family of Halhead in Aberdeenshire, who was likewise settled in the same trade, and at the same place, and in consequence of his success, was enabled to purchase, with additions, the estate of his elder brother,

brother, greatly encumbered by the debts of those who had possessed it for some generations preceding. The mother of JOSEPH BLACK, and the mother of JAMES RUSSEL, late Professor of Natural Philosophy in the University of Edinburgh, were sisters; and the mother of ADAM FERGUSON, Professor of Moral Philosophy, was their aunt; a circumstance which was the origin, though not the cement, of a friendship subsisting between them through life.

WHILE Mr BLACK, the father, lived at Bourdeaux, the great MONTESQUIEU, being President of the Parliament or Court of Justice in that province, honoured Mr BLACK with a friendship and intimacy, of which his descendants, to this hour, are justly proud. They preserve letters, or scraps of correspondence, that passed between the President MONTESQUIEU and their ancestor, as they would titles of honour descending in their race. On a paper wrapped round a bundle of such letters, the following note is found in the handwriting of JOSEPH BLACK. “ My father was honoured with President MONTESQUIEU’s friendship, “ on account of his good character and virtues. He had no “ ambition to be very rich; but was chearful and contented, “ benevolent and liberal-minded. He was industrious and prudent in business, of the strictest probity and honour, very “ temperate and regular in his manner of life. He and my “ mother, who was equally domestic, educated thirteen of their “ children, eight sons and five daughters, who all grew up to “ men and women, and were settled in different places. My “ mother taught her children to read English, there being no “ school for that purpose at Bourdeaux.”

So much may suffice for an account of JOSEPH BLACK’s parentage. No words, added to those used by his son, to delineate the father’s character, can improve it; and nothing more is wanting, to account for the regard with which he was honoured by the President MONTESQUIEU. This illustrious personage, together

together with a great simplicity of heart in himself, had a glowing sense of modest merit in others, and a partiality also for the manners and institutions of the British nations, which he thought singularly happy, and in respect of which, he willingly listened to any important details. On being made acquainted with BLACK's intention of leaving Bourdeaux, he wrote to him a letter, in which, with other expressions of kindness, are the following: "I cannot be reconciled to the thoughts of your leaving Bourdeaux. I lose the most agreeable pleasure I had, that of seeing you often, and forgetting myself with you \*."

A FEW years before Mr BLACK retired from business, his son JOSEPH, in the year 1740, being about twelve years of age, was sent home in order to have the education of a British subject; and having, for some years, gone to school at Belfast, or its neighbourhood, was sent to continue his education at Glasgow College, where he passed through the ordinary course of preparation for any of the learned professions to which he might devote himself; and upon his father's requiring him to make a choice, he took to medicine, as agreeable to the habit of physical studies which he had already acquired.

IN entering on this pursuit, he became connected with the celebrated WILLIAM CULLEN, M. D. then Professor of Medicine at Glasgow, and was distinguished through life by this ingenious man with an uncommon degree of friendship and confidence, which he did not fail to acknowledge and to return. In continuing the same studies afterwards at Edinburgh, he, as well as ADAM FERGUSON, lived with their relation JAMES RUSSEL; whose singular correctness, and precision of thought, in various branches of science, could not fail to be of use to all who approached him.

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BEFORE

\* " JE ne me fais point à l'idée de vous voir quitter Bourdeaux. Je perds le plaisir le plus agréable que j'eusse, qui étoit de vous voir souvent, et de m'oublier avec vous."

BEFORE JOSEPH BLACK offered himself a candidate for a degree in medicine, he had already made his discovery of Fixed Air; that is to say, of an elastic fluid, which being fixed in calcareous and alkaline substances, is dispelled from them in their calcination by fire, or effervescence with acids, leaving a residuum, which, in the absence of this air with which it had been combined, becomes caustic; a quality which it retains until the air of which it was deprived is again restored. Pure chalk, or pure calcareous matter of any form, in this experiment, loses about two-fifths of its weight, or, more accurately, forty-one parts in the hundred. Here, accordingly, is a ready way of assaying limestone or marl, to ascertain its purity or its value, to the husbandman in particular, who would employ it to improve his soil.

THE air obtained in this experiment has its peculiar qualities. Besides that of being fixable in stone, it is heavier than common air, its specific gravity being nearly double. Flame is extinguished, and animals are suffocated in it equally as in water. By this discovery, notwithstanding the ingenious and copious observations of Dr HALES and others, on the subject of what were called Factitious Airs, it was reserved for BLACK to give a spur and a new direction to the researches of science.

FROM this beginning, great progress has been made in distinguishing varieties of elastic fluid, and in ascertaining the composition of the atmosphere itself. Chemistry was become a favourite study in France, as it had long been in Germany, and the report of a new species of air was every where received with avidity. The experiments of BLACK were repeated, and the inferences confirmed by ingenious men both at home and abroad; though by some in Germany the result was at first contested, and a controversy arose, in which it does not appear that BLACK took any part, nor does any doubt now remain of the fact, as stated by him, or of the inferences he drew from it\*.

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\* Vid. *Opuscles Physiques et Chimiques* par M. LAVOISIER.

AN elastic fluid, indeed, of the same qualities, has been since obtained from various sources; from vinous fermentation, from the earth itself, laid open in grottos or fissures, from mineral springs, from the breathing of animals, and from the combustion of charcoal. This air is found to have the properties of an acid; and the French chemists, being led by their experiments to consider it as produced by the union of the principle of charcoal with vital air, have laid aside the appellation of Fixed Air, and have substituted that of Carbonic Acid Gas in its stead.

IF the researches to which the observation of fixed air gave occasion, had terminated here, the accession to science, though important, had been comparatively small. The composition of the atmosphere, the distinction of vital and mephitic air, the effects of respiration, and the conjectural theories of combustion and animal heat, might have remained unknown.

HERE, however, no mean progress has been made, and names too numerous to be recited in such minutes as these, will, on account of their part in these studies, go down, with well-merited lustre, to future ages. Nations may hereafter contend for the honour of such discoveries as were made by LAVOISIER, PRIESTLEY and others \*, not merely of single or insulated facts, but of a magnificent order existing in nature, for the continuance of vegetation and animal life. Even France and England, though

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rich

\* To LAVOISIER we owe the discovery, that atmospheric air is not homogeneous, but composed of nearly three parts of azot, which is not of any effect in respiration, otherwise than as it dilutes the remaining fourth part, or the vital air, which, without being so diluted, would be too intense either for the purpose of animal respiration or of common fire.

To PRIESTLEY we owe the discovery, that the well-known corruption or waste of vital air, in the burning of fuel, or the respiration of animals, is repaired in the vegetation of plants.

If in science there might be any choice of truths, I would willingly hope that the decomposition of water may be found a mistake.

rich in the treasures of science, and each having much to boast of its own, may, in the future, as in times past, be jealous of what is gained by its neighbour; it is pleasant, however, to observe, that the individuals chiefly concerned were untainted with such illiberal passions. LAVOISIER, in sending to BLACK a copy of his experiments on respiration, tells him: "It is but just you should be one of the first to receive information of the progress made in a career which you yourself had opened, and in which (he says) all of us here consider ourselves as your disciples \*." To this BLACK replied, with a just admiration of what the French chemists were doing, and, as might be expected from his modesty, without reference to any merit of his own.

NEARLY about the same time that BLACK made the discovery of fixed air, his mind was already at work on the phenomena of latent heat. Among his papers are found some note-books, in which he had inserted observations and queries, medical, chemical, and miscellaneous; accompanied with particular references or allusions to times and circumstances, which serve to fix dates within certain limits at least. In one book, the notes appear to have been written while he was a student at Edinburgh, or candidate for his degree, in the year 1756; at which time he made or published his discovery of fixed air. In this book are contained conjectures and queries relating to the cold produced in the liquefaction of salt and snow, and in general in the solution of salts in water. Seeming to have the doctrine of latent heat in his view, "Is it not," he says, in one place, "owing to this that all bodies, in becoming fluid, have occasion for more heat than in their solid state?" In another place he says,  
 "Does

\* "Il est bien juste, Monsieur, que vous soyez un des premiers informés des progrès qui se font dans une carrière que vous avez ouverte, et dans laquelle nous nous regardons tous comme vos disciples."



"Does not the heat produced in the slacking of quicklime, arise from the sudden fixing a part of this fluid (water) into a solid state? Is not this the case in the setting of gypsum?" In the same way he accounts for the intense cold produced by ice and nitrous acid, applies the principle again to all saline solutions, and ascribes the slow granulation of salt to the re-appearance of heat. "Is not ice," he says, "crystallized water? and does it not always feel cold, because it melts on our handling it? This is similar to the solution of salt in water." These notes, from circumstances intermixed, appear to have been written as early as 1756. In a following note-book, which, from circumstances also, does not appear to be of a later date than the year 1757, there are several queries, of the same nature with those above mentioned, particularly with respect to the curious observation of FAHRENHEIT, that water, if not disturbed, will cool below 32 degrees without freezing, but the moment it is disturbed, it raises the thermometer to 32 degrees, and freezes. "Is not this," he says, "the heat that is unnecessary to ice?" In the same note-book conjectures are carried still farther, and applied to the production of vapour. A way is mentioned to estimate the quantity of heat which is employed and disappears in the formation of vapour, and cannot afterwards be discovered by the thermometer. "Place a phial," he says, "with water, close-corked in a stove; open it suddenly, and see how much of it is converted into vapour, while the water comes down to the boiling point." Observations to the same effect are continued through six small note-books; in different places of which, it is observed, that animal heat originates in the lungs, and that heat always accompanies the production of fixed air.

FROM these notes, it appears, that the experiments in which BLACK evinced his doctrine of latent heat to the satisfaction of his pupils, were not made at random, or without previous apprehension of the fact, as it appeared in the ordinary course of nature

ture or chemical phenomena. "Heat," he observed, "is in nature the principle of fluidity and evaporation, though, in producing these effects, it is latent in respect to the thermometer, or any sensation of ours; and as matter, otherwise quiescent, becomes voluble and volatile in liquid and in vapour, heat may be considered in nature as the great principle of chemical movement and of life. If it pass through vacuity as well as through body, as it certainly does in its communication from the sun to the planets, we must consider it not as an accident in bodies, but as a separate and specific existence, not less so than light or electric matter\*; and though agreeing with these in some of its effects, in its nature possibly different from either." But such was BLACK's caution not to outrun the course of actual evidence, that he declined any discussion of the question, relating to the absolute nature of this magnificent power in the system of nature.

My reading in chemistry does not enable me to say, how far the doctrine of latent heat is, in these precise terms at least, admitted as a principle in the received theories of combustion and animal heat; but, to my limited apprehension, it appears to be the only solid foundation of any theory that proceeds upon the supposed decomposition of *ignifying* or vital air, manifesting a light and heat previously latent in such air. We can have no direct proof of latent heat in the atmosphere, or permanently elastic fluids, and it is from analogy only that we assume it to exist in such fluids. The maxim of NEWTON, indeed, may be applied here, that what is uniformly observed in any department of nature, as far as our experience reaches, may be safely deemed general within such department; and the heat which we find disappear

\* It is no doubt a mighty increment in science, to have found such powerful substances operating, as the writer of these minutes apprehends, without gravitation, inertia, or impenetrability, the great bases and columns of the mechanical philosophy.

appear in liquefaction and evaporation, we may safely assume as existing also, though in a latent state, in every fluid of a similar form. Even BLACK admitted, that in the atmosphere, under all the manifest changes of its temperature, there may be, and probably is, a very great measure of latent heat.

SUCH were the scientific discoveries of BLACK, when residing as a student at Glasgow or at Edinburgh, at least before his nomination to a professorship at either place. When his friend Dr CULLEN was removed from Glasgow to Edinburgh, BLACK was appointed to succeed as Professor of Medicine at the former place; and following his example as lecturer on Chemistry also, was, from this time forward, more employed in detailing particulars already known, for the information of his pupils, than in pursuing any series of investigation for himself.

IN entering upon this task, it became matter of course to define the science, and even in this our Professor gave a specimen of his character, and the modesty of his pretensions, in a matter formerly enveloped in mystery, and the affectation of magical power.

THE science of nature, indeed, is interesting in all its branches, whether they rise into the heavens, or sink into the bowels of the earth. We are placed in a busy scene, and have our safety and accommodations at stake, in the midst of operations and changes that greatly affect them. The principles that operate in our system are the objects of our science, and when known, become the instruments of our art, towards procuring what it has pleased Providence to make the conditions of our preservation and well-being. In respect to these principles, science becomes an accession of skill to every manufacturer, and to every labourer of the soil, as it is to the contemplative an opening into the interior springs, which are employed in the production of so much beauty and order in the system of nature around us; and this is the rank of importance, which, without the pretensions of alchemy,

alchemy, or the mysteries of supernatural power, the chemist might assume to his studies. But, in these, BLACK still saw no more than research and inquiry, far short of complete science or comprehensive system, and he characterized the study on which he was to enter, by the use of its principal instruments, Mixture and Heat. If discoveries have since raised it to a higher rank among the branches of natural science, he himself has contributed his share in the progress it has made. He had published his discovery of fixed air, in a pamphlet, about the time that he obtained his degree in medicine; but that of latent heat was never otherwise published than as a part of his academical course. His pupils, indeed, being numerous, and from different parts of the new, as well as the old world, made his doctrine sufficiently known\*.

AFTER a few years employed by CULLEN in the first profession to which he was appointed at Edinburgh, he was removed to a different branch of the medical school; and BLACK, as before, being called to succeed him, was, on the 17th of April 1766, admitted Professor of Chemistry at Edinburgh. His talent for communication being noway inferior to that which he possessed for observation and inference, his manner of acquitting himself in his new situation gave additional lustre to the schools of science, and rendered him one of the principal ornaments of the University, in which he continued, for about thirty years, to teach the different branches of chemistry, with a reputation always increasing. His style was characterized by the most elegant simplicity. His address in performing experiments was remarkable, and

\* THE writer of this article has little more than heard of chemistry as a branch of general science, and fondly embraced any doctrine as it seemed to connect with the system of nature; but farther, his own studies have been so different, that he would not, if he could, charge his mind with any of its practical details; and he pleads the reader's indulgence, if, under this defect in treating of BLACK, he hastens to subjoin a description of the man to so imperfect an account of his science.

and in the impression he made, subjects perplexed or intricate became perspicuous and clear of superfluous or questionable matter. To the last, and under symptoms of declining health, his mind gave proofs of strength undiminished. In speaking, his voice, though low, had an articulation which made him be distinctly heard through every corner of a spacious hall, crowded with some hundreds of his pupils: and the simplicity of his expression, if not eloquence, had, to those who listened for information, something more engaging and powerful than any ornament of speech could produce.

AVERSE to hypothesis or vain conjecture, his science was a just comprehension of facts, and might be adopted by any artist who consulted him, with as much safety as he relied on any practice he himself had experienced. His time and attention being devoted to the communications which his pupils had a right to expect from him, very much limited his practice as a physician. But where he was called or could attend, his manner was singularly acceptable. Without flattery, or uncommon pretensions to skill, he won the confidence of his patients, and, with unaffected concern for their benefit, was often successful in mitigating their sufferings, if not in removing their complaints. He was, in short, a physician of great repute, in a place where the character of a physician implies no common degree of liberality, propriety, and dignity of manners, as well as learning and skill. Never being anxious to bring himself forward into public view, little may remain with posterity to distinguish him as an author, unless his executors should think proper to publish the notes from which he used to prelect, and which, notwithstanding the subsequent progress of science, may still be of use to the student, as a most solid foundation on which to proceed in his studies.

AMONG the few things published by himself in his own time, we may reckon his Thesis or inaugural essay, *De Acido a Cibis*

*orto, et de Magnesia Alba:—Experiments on Magnesia, Quicklime, and other Alkaline Substances*, printed between the years 1754 and 1756:—*Observations on the Effect boiling has upon Water, in making it freeze more readily*, inserted in the sixty-fifth volume of the Transactions of the Royal Society of London, for the year 1774:—*Analysis of the Waters of some Hot Springs in Iceland*, made at the request of his friend Mr STANLEY, who brought the specimen for his inspection; inserted in the third volume of the Transactions of the Royal Society of Edinburgh:—*Two Letters*, one published in the year 1784, by Professor CRELL, in the tenth volume of his Collections; the other addressed to Monsieur LAVOISIER, and published in the *Annales de Chimie*.

WITH these notes of our Author's professional distinction, readers will not expect any tale of adventures or remarkable events, which the lives of studious persons seldom afford; and we must conclude with a few lines of character; such as those who are placed in like circumstances may understand as spoken of themselves, and for their advantage.

HIS aspect was comely, his manner unaffected and plain, and as he never had any thing about him for ostentation, he was at all times precisely what the occasion required, and nothing more. Much as he was engaged in the details of his public station, and chemical exhibitions, his chamber never was lumbered with books, specimens of natural history, or the apparatus of experiments; nor did any one ever see him hurried at one time to recover any matter which had been improperly missed at a former. Every thing being done in its proper time and place, he ever seemed to have leisure in store, and he was ready to receive his friend or acquaintance, and take his part with complacence, in any conversation that occurred. No one ever, with more ease to himself, refrained from professional discussions of any sort, or, in mixed company, more willingly left the subject of conversation

it to stand out to stand out to

to be chosen by others. Many years member of a society \* composed of men of the highest rank, of Judges, lawyers and military men, as well as citizens and professed men of letters; he kept his place with propriety, and in this group never betrayed any awkward peculiarity of his education or calling. His more intimate associates and friends, besides those of his own profession CULLEN and MONRO, were JAMES WATT, now of Birmingham, an engineer of the first order, whether ancient or modern, and who has happily united, in the grounds of his art, chemical as well as mechanical science; Mr GEDDES of the Glass-works at Leith, an eminent manufacturer, who well knew the value of his friend's suggestions. Such intimates as these could scarcely allow BLACK to relax from the toils of his professional studies. But he had others, for whom his profession had no charm, and whose attachment to him, as to one another, arose from the experience of ingenuity and candour, rather than the identity of studies, or agreement of opinions. Such were DAVID HUME, Esq; Dr ADAM SMITH, JOHN HOME, ALEXANDER CARLYLE; and JOHN CLERK of Eldon, of whom the last, without having ever been at sea, instructed the navy itself in naval tactics. At the head of either list, however, in respect to BLACK's habits of intimacy, ought, perhaps, to have been placed JAMES HUTTON, who made up in physical speculation all that was wanting in any of the others. It may be difficult to say, whether the characters of BLACK and HUTTON, so often mentioned together, were most to be remarked for resemblance or contrast. Both profound in

P 2

physical

\* THIS society formed itself, about the year 1770, upon a principle of zeal for the MILITIA, and a conviction that there could be no lasting security for the freedom and independence of these islands, but in the valour and patriotism of an armed people. It became known, by some whimsical accident, by the name of the *Poker Club*.

physical science; and rigid adherents to fact, in exclusion of hypothesis or vain conjecture. Both of consummate humanity and candour. BLACK was serious, but not morose; HUTTON playful, but not petulant. The one never cracked a joke, the other never uttered a farcasm. BLACK was always on solid ground, and of him it might be said, *Nil molitur ineptè*. HUTTON, whether for pleasantry or serious reflection, could be in the air, speculate beyond the laws of nature, and treat the common notion of body, with its magnitude and figure, as a mistake. In these speculations BLACK never took any part, farther than to be diverted with any play of fancy, or refinement of thought, which, he well knew, in the case of his friend HUTTON, did not preclude the use of correct and sober reason when the subject required it, or was within its cognisance. The researches of HUTTON were unremitted, and his reference to the order and arrangement of this, which he called a Living World, was judicious and happy \*. Unreal as corporeal subjects were in his apprehension, he established a lucrative manufacture, on principles of chemistry, and was for many years of his life keenly employed in the practice of agriculture. To this he was led by becoming a proprietor of land on the death of his father, when he hastened to Norfolk, where he had formerly lived with a farmer, to observe the husbandry of that country. There he purchased a plough, hired a ploughman, and brought both on the post-chaise with him to Berwickshire. The neighbours were diverted with this assortment of company and baggage, and no less with the attempt which followed, to plough with a pair of horses without

\* He, too, had carried his studies so far as to obtain a degree in medicine; but an attempt to consult or see him would have been met with a laugh, or some ludicrous fancy, to turn off the subject.



out a driver. This joke, however, has become serious, and is now the general practice from one end of Scotland to the other\*.

IN returning to BLACK from this enumeration of his intimate friends, serving, perhaps, to disclose his character no less than any other circumstance relating to him, we but hasten to the conclusion of a life no less distinguished by correctness and propriety of conduct, than by ingenious reasoning, and scientific research. Fully entitled to the appellation of *Frugi*†,—that seemingly cant expression, of which the old Romans were so fond,

\* HAVING said so much of HUTTON in this occasional notice, so far short of his merits, it may not be improper to prepare those who may consult him as an author, to meet with a disappointment for which his friends could never rightly account. Though uncommonly luminous and pleasant in conversation, he was obscure, unintelligible, and dry in writing, to an equal degree. His favourite specimens of natural history, he used to say, were God's Books, and he treated the books of men comparatively with neglect. This may, in some measure, account for his want of style or his indifference to language. In company, he spoke to be understood by such as were present, and when obscure, was called upon to explain himself. But alone, he was not aware that others could be at a loss for a meaning so clear to himself. From this circumstance, (notwithstanding many volumes written in the last years of his life, more numerous, perhaps, than all he ever read that were written by others, except the voyages and travels, from which he was perpetually collecting facts to complete his view of the terrestrial system), his very ingenious conceptions, to be received as they ought, must come from some other pen than his own.

† On this subject we may consult the following passage of CICERO, *Tusc. Quæst.* lib. iii. c. 300. "Sed quia, nec qui, propter metum, præsidium reliquit, quod est Ignaviæ; nec qui, propter avaritiam, clam depositum non reddidit, quod est Injustitiæ; nec qui, propter temeritatem, malè rem gessit, quod est Stultitiæ, *Frugi* appellari solet. Eas tres virtutes, Fortitudinem, Justitiam, Prudentiam, frugalitas est complexa; etsi hoc quidem commune est virtutum: omnes enim inter se connexæ et jugatæ sunt. Reliqua igitur et quarta virtus, ut sit ipsa Frugalitas. Ejus enim videtur esse proprium, motus animi appetentis regere et sedare; semper adversantem libidini, moderatam in omni re servare constantiam, cui contrarium vitium Nequitia dicitur."

fond, that they used it to denote the foundation, or the summary of all the virtues, he carried into his private affairs the same order and good conduct which he employed in his professional duties ; and he reaped, through life, the benefit of his attention to this particular, in the ease of his circumstances, and in the power it gave him, on occasion, to assist a friend, or contribute to the attainment of any public convenience. From those, indeed, who can mistake remissness or improvident waste for generosity, BLACK may have incurred the imputation of penury. But the proofs of this charge, if ever it were brought, in his case were not to be distinguished from the effects of sound reason and good sense. No one ever struck the proper medium more exactly than he did. His expences were regulated, but nowise forbid, or unbecoming his station. His house was spacious ; and his table, at which he never improperly declined any company, was elegant and plentiful, rather above than below his condition. His contributions for public purposes were liberal, and his purse was always open to assist a friend. Much of his practice as a physician arose from his previous connection with the patient as a friend ; and he was as assiduous where he would not accept, or where he could not expect a fee, as in the most lucrative part of his profession.

His own constitution never was robust ; and every cold he caught, or any approach to repletion, affected his breast so much as to occasion a spitting of blood. This he guarded against, by restricting himself to a moderate or abstemious diet. As his infirmities increased with age, he met them with a proportional attention and care, regulating his food and exercise by the measure of his strength ; and thus preventing the access of disease from abroad, he enjoyed a health which was feeble, but uninterrupted, and a mind undisturbed in the calm and chearful use of his faculties. A life so prolonged had the advantage of present ease, and the prospect, when the just period should arrive, of a calm dissolution.

tion. This accordingly followed, on the 26th of November 1799, and in the seventy-first year of his age, without any convulsion, shock, agitation or stupor, to announce or retard the approaches of death. Being at table, with his usual fare, some bread, a few prunes, and a measured quantity of milk diluted with water; and having the cup in his hand when the last stroke of his pulse was to be given, he appeared to have set it down on his knees, which were joined together, and in this action expired, without spilling a drop,—as if an experiment had been purposely made, to evince the facility with which he departed. So ended a life which had passed in the most correct application of reason and good sense to all the objects of pursuit which Providence had prescribed in his lot. His effects, when looked into, shewed how much he had profited by the order and just arrangement he had ever maintained in his affairs; amounting almost to double of what any one thought his income or his frugality could have enabled him to acquire. The whole was disposed of by will, without specifying any sum, in a neat and satisfactory manner. Being divided into ten thousand shares, it was parcelled to a numerous list of relations, in shares; in numbers, or fractions of shares, according to the degree in which they were proper objects of his care or solicitude.

THIS account of JOSEPH BLACK, however inadequate to the merit of its subject, is inscribed to the President and Members of the Royal Society of Edinburgh,

By their faithful Associate,

And humble servant,

ADAM FERGUSON.

HALLYARDS, in Tweeddale, }  
23d April 1801.

tion. This accordingly followed, on the 28th of November 1799, and in the seventh year of his age, without any previous illness, shock, agitation or stupor, to announce or attend the approaches of death. Being at table, with his usual calm, serene, and unaffected demeanour, he continued drinking with water; and having the cup in his hand when the last stroke of his pulse was to be given, he appeared to have it in down on his knees, which were joined together, and in this position, without striking a drop, as if an experiment had been made, only made, to revive the fluid, with which he departed. So ended a life which had passed in the most correct application of reason and good sense to all the objects of human knowledge, and was finished in his lot. His efforts, when he had seen how much he had profited by the order and plan of management he had ever maintained in his affairs; amounting almost to a knowledge of what any one thought his income or his strength could have enabled him to acquire. The whole was directed by a will without effort, trying any thing in a new and still better manner. Being directed into the grand things, it was parcelled to a number of relations, in things in numbers, or fractions of things, according to the degree in which they were proper objects of his care or solicitude.

The account of Mr. Black, however inadequate to the merit of his subject, is indebted to the President and Members of the Royal Society of Edinburgh.

By their faithful Affection,

And humble Obedience,

ADAM FRUGO

Printed in Edinburgh, by James Ballantyne, in the Strand, 1799.

## A P P E N D I X.

*LIST of MEMBERS or FELLOWS of the ROYAL SOCIETY of  
EDINBURGH, continued from the Fourth Volume. [History of  
the Society, Appendix.]*

THE following Members were elected at the General Meeting,  
Jan. 28. 1799.

Members chosen,  
Jan. 28. 1799.

### RESIDENT.

The Reverend *Thomas Macknight*, one of the Ministers of South  
Leith. L.

*William Robertson*, Esq; one of the Keepers of the Records of  
Scotland. L.

### NON-RESIDENT.

The Right Honourable Lord *Webb Seymour*. P.

*Gavin Hamilton*, Esq; Calcutta. L.

*Henry Stuart*, Esq; of Alanton. L.

June 24. 1799.

Members chosen,  
June 24. 1799.

### RESIDENT.

Sir *George Mackenzie* of Coul, Baronet. P.

Mr *Robert Jamieson*, junior, of Leith. P.

### NON-RESIDENT.

*Matthew Baillie*, M. D. London. P.

*John Lind*, M. D. Physician to the Royal Hospital at Haslar. P.

Mr *William Blizard*, F. R. S. London, Surgeon to the London  
Hospital. P.

Mr *Thomas Blizard*, Surgeon to the London Hospital. P.

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Q

Jan.

Members chosen,  
Jan. 27. 1800.

Jan. 27. 1800.

RESIDENT.

*William Arbuthnott, Esq; L.*

Members chosen,  
June 23. 1800.

June 23. 1800.

RESIDENT.

*Mr Alexander Christison, one of the Masters of the High-School,  
Edinburgh. L.*

*Gilbert Innes, Esq; of Stow. L.*

*Walter Scott, Esq; Advocate. L.*

*The Reverend Andrew Bell, D. D. P.*

NON-RESIDENT.

*James Fellows, M. B. Physician to the Army. P.*

Members chosen,  
Jan. 26. 1801.

Jan. 26. 1801.

RESIDENT.

*Alexander John Alexander, Esq. L.*

NON-RESIDENT.

*Benjamin, Count Rumford. P.*

Members chosen,  
Jan. 25. 1802.

Jan. 25. 1802.

RESIDENT.

*William Porter, Esq; of the Russian Factory. L.*

*Lieutenant-Colonel David Robertson, Deputy Adjutant-General,  
Ceylon. P.*

NON-RESIDENT.

*Francis Gregor, Esq; M. P. of Cornwall. P.*

*Sir William Ouseley. L.*

Jan.

Jan. 31. 1803.

Members chosen,  
Jan. 31. 1803.

RESIDENT.

The Right Honourable Lord *Minto*. L.  
Reverend Dr *Jamieson*, Edinburgh. L.  
*Thomas Telford*, Esq; Civil Engineer. P.

June 27. 1803.

Members chosen,  
June 27. 1803.

RESIDENT.

Mr *James Bryce*, Surgeon, Edinburgh. P.  
Reverend Dr *Brown*, Professor of Rhetoric, Edinburgh. L.

Jan. 30. 1804.

Members chosen,  
Jan. 30. 1804.

NON-RESIDENT.

Mr *William Wallace*, Professor of Mathematics in the Royal Military College, Marlow. P.  
Dr *James Robertson*, Physician in Barbadoes. P.  
The Reverend *Jonathan Boucher*, A. M. F. A. S. London, Vicar of Epfom, in Surry. L.

June 25. 1804.

Members chosen,  
June 25. 1804.

RESIDENT.

Lieutenant-General *Richard Vyse*. P.  
*William Forbes*, Esq; P.  
*Alexander Irving*, Esq; Advocate, Professor of Civil Law, Edinburgh. P.

NON-RESIDENT.

*John Rutherford*, M. D. P.

## List of Donations.

*DONATIONS made to the ROYAL SOCIETY OF EDINBURGH, since 1797.*

From Dr *Joseph Black*, Professor of Chemistry in the University of Edinburgh, F. R. S. Edin.,

The Collection of Fossils of the late Dr *James Hutton*, F. R. S. Edin. May 1798. See History of the Society, p. 39.

From Mr *J. Rennie*, Civil Engineer, F. R. S. Edin.

Reports of the late Mr *John Smeaton*, F. R. S. made in the course of his employment as an engineer. 1797.

From the Academy of Geneva.

*Voyages dans les Alpes*, par H. Benedict de Saussure, tom. iii. et iv. 1796.

*Essai sur l'Hygrometrie*, par le même. 1783.

*Defense de l'Hygrometre à Cheveu*, par le même. 1788.

*Essais Philosophiques de feu Adam Smith, &c. Traduits par M. Prevost, Prof. de Philosophie à Geneve, de l'Acad. de Berl. et de Societ. Royale d'Edimbourg.* 1797.

From Mr *Baron Masferes*.

*Scriptores Logarithmici.* 4 vol. 4to. 1801.

From



From Sir W. Ouseley.

Persian Lyrics from the *Diwan-i-Hafiz*. By J. Haddon Hindley. 1800.

From the Author.

Description and Use of the Sliding Rule, by Andrew Mackay, LL. D. F. R. S. Edin. 8vo. 1799.

From the Author.

Essay on the Comparative Advantages of Vertical and Horizontal Wind Mills, by R. Beatson, Esq; F. R. S. Edin. 8vo. 1798.

From the Author.

*Des Signes et de l'Art de penser, considérées dans leur Rapports Mutuels*. J. M. Degerando. 4 vols. 8vo. 1800.

From the Right Honourable Sir J. Banks.

*Catalogus Bibliothecæ Historico-Naturalis* Josephi Banks. Auctore Jona Dryander, Reg. Soc. Bibliothecario, tom v. 1800.

From the Editor.

Lectures on Natural and Experimental Philosophy, by the late George Adams, Mathematical Instrument-maker to his Majesty. 2d Edition, by Mr Jones, Mathematical Instrument-maker. 5 vols. 8vo. 1797.

From the Linnean Society, London.

Transactions of the Linnean Society, vol. i, ii, &c. vol. v. 1800.  
Also Charter of the Linnean Society, 4to.

From

From the Asiatic Society.

Asiatic Researches, or Transactions of the Society instituted in Bengal for inquiring into the History, Arts, Sciences, Literature, &c. of Asia. 7 vols. 4to. 1801.

From the Author.

Illustrations of the Huttonian Theory. 8vo. 1802.

From Dr Blane, London.

Inquiry into the Causes and Remedies of the late and present Scarcity of Provisions, in a Letter to the Right Honourable Earl Spencer. 8vo. 1800.

From the Author.

*Dissertatio Inauguralis de Necrosi ac Callo.* Auctore Alex. Hermannno Macdonald, *Batavo.* 8vo. 1799.

From the Author.

The Morbid Anatomy of some of the most important parts of the Human Body, by *Matthew Baillie*, M. D. F. R. S. London, 2d edition. 8vo. 1797.

From the Author.

Series of Engravings, intended to illustrate the Morbid Anatomy of the Human Body, 10 Fasciculi, large 4to, from 1799 to 1802. By *Matthew Baillie*.

From the Royal Society of London.

The Transactions of the Society, from 1797 to 1803 inclusive.

From

From the Author.

Essays, Literary, Political, and Economical, in 2 vols. by *J. Gardiner*, M. D. Fellow of the Royal College of Physicians, and of the Royal Society of Edinburgh. 2 vols. 8vo. 1803.

From the Council of the Interior of the Batavian Republic.

*Flora Batava*, 8 *Livraisons*, *J. G. Sepp et Fills*. 1803.

From the American Society.

Transactions of the American Philosophical Society at Philadelphia. 4 vols. 4to. 1799.

From the Imperial Academy of Peterfburgh.

*Nova Acta Acad. Scientiarum Imperialis Petropoliana*, tom. ix, x, xi, xii, xiii. 1802.

From Baron *Maseres*.

Tracts on the Resolution of adfected Equations by various methods of Approximation. Published by *Francis Maseres*, Esq; F. R. S. London Baron of Exchequer. 8vo. 1800.

From the Translator, *P. B. counin*, F. R. S. Edin.

*De l'Etat Politique de l'Europe avant et après la Revolution. Tra-  
duit de l'Allemand de F. GENZ.* 1803.

From the Author.

Memoir on a Subterraneous Graphometer, invented by *John Komarzewski*, formerly Lieutenant-General in the service of the King and Republic of Poland. 1803.

From

From Major Rennel.

African Researches, or Proceedings of the Association for promoting the Discovery of the Interior Parts of Africa, vols. i. & ii. 1802.

From the National Institute.

*Mémoires de l'Académie des Sciences*, 1789 & 1790.

END OF THE FIFTH VOLUME.









